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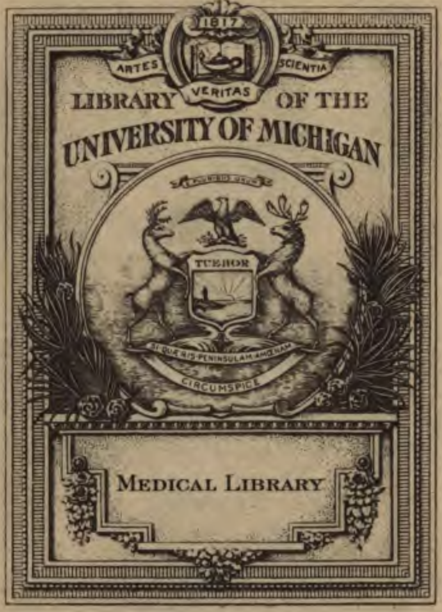

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THE

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EXHIBITING

A CONCISE VIEW

OF THE

LATEST AND MOST IMPORTANT DISCOVERIES

IN

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THE
EDINBURGH
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1. JANUARY, 1826.

PART I.
ORIGINAL COMMUNICATIONS.

I.

Of Squamous Disorders. By E. W. Duffin, M. D. F.R.C.S.E.

Though much has been written on cutaneous diseases, the study may still be considered as in its infancy, when compared with the advanced pathology of other morbid affections. Their treatment has indeed largely participated in the general improvements of the art of healing; but there still remain many varieties so extremely refractory, as to require no apology for our attempts to improve their theory or management. The squamous class of disorders in particular, which we are about to consider, is entirely of this description; and from the over refinement of nosologists, and the prevalence of these diseases in barbarous ages and nations, they are still perplexed by a greater mass of obscure or inappropriate matter than most of the others. It has for this reason chiefly been found necessary here to treat the subject with rather more systematic exactness than the novelty of the matter, or practice communicated in the following article, may seem to justify.

A morbidly harsh, or scabrous, hard, thickened condition of the skin, accompanied by a desquamation of thin laminæ of cuticle, or a separation of whitish opaque scales, leaving either a natural and healthy, or a reddened, smooth, and glistening surface underneath, embraces in general terms the characteristic features of squamous disorders.

This class of diseases is one of the most frequent and troublesome to which the skin is liable, occurring, at least in Britain, at all periods of the year, and under every circumstance of life.

In the practice of the Dispensary for the cure of cutaneous diseases in this city with which I am connected, the cases of psoriatic lepra during the last year, far outnumbered those of common itch; but I believe that it is, upon the whole, a disease more common in Scotland, especially in the northern parts, than in England.

There is reason to believe that the functions of the skin are either materially impeded, or entirely suspended, in a greater degree in proportion to the extent and severity of squamous diseases, than in any other class; hence their importance in a physiological point of view must be obvious. But were there no other inducement to attend to them, than the unsightly appearance which they produce, the unpleasant sensations they in some cases occasion, and their acknowledged difficulty of cure, they are, on these accounts, well deserving more patient investigation than they have ever received, unless perhaps in the labours and researches of Dr Willan.

Notwithstanding the derangement of structure that squamous diseases produce in the cutaneous texture, so that the perspiration, in particular, must be either considerably diminished or entirely suppressed in those parts which are affected, it is still true that the health of the general system is very rarely influenced by the local disease, unless in extreme cases.

On the contrary, those persons who are affected with *Lepra*, are generally of the most vigorous and healthy constitution. In Pellagra, however, a species of pityriasis common in the fertile plains of Lombardy, the most distressing and destructive constitutional symptoms are produced in the course of the external disease, or accompany it from the commencement. Indeed it is a matter of surprise, when an organ, so intimately connected by continuity of surface with the internal superficies of the body, and subservient to so many important offices as the skin, is diseased in its texture, that the general health can escape injury; and it is perhaps only by supposing that the lungs, and probably the kidneys perform extra duty, (the one in exhaling an increased quantity of watery vapour, and the other in secreting a preternatural flow of urine,) that we are at all enabled to explain the want of influence that squamous diseases seem to possess in their relation to the general health of the animal economy, provided, as we have stated to be highly probable, the absolute quantity of daily perspiration be considerably diminished.

No age or sex is exempt from the disorders under consideration, though they may be said to occur most frequently, and are less tractable in children, and in persons who have passed the meridian of life, than in the middle aged; and women, upon the whole, are more subject to them than men.

No class perhaps of cutaneous diseases varies more in extent and appearance than the *Scaly*; the morbid parts may be confined to a few patches, not larger than a split pea, accompanied by the slightest possible branny desquamation, or they may traverse over the whole surface, covering in some cases five-sixths of the area of the body at one time, and be attended by so abundant an exfoliation of thick dense scales, that every motion is performed with a crackling noise, and in the course of a night the sheets will be covered with separated scales.

Scaly diseases, under every modification, are always tedious and difficult to cure, and even when removed by the aid of medical treatment, are more prone to return at certain seasons, or from comparatively trivial causes, than any others. Without medical interference they generally continue for months, or even years, nay in many instances, through an active and long life to death. *(Lorry de Lepra, page 367.)*

But what renders them still more perplexing is their obstinacy, and frequent duration for months after the evident exciting cause has been removed.

They seem to be as much as any other class of diseases under the influence of the season, and are particularly liable to aggravation or recurrence during the spring and autumn months, when vicissitudes in the state of the atmosphere are most sudden and severe; but in respect to extent and severity of the returns, they differ much in different individuals.

The precise states of the system under which they occur have not been satisfactorily ascertained; and it is obvious they must remain problematical in all cases where the disease exists in the same individual from childhood to an advanced age, under every vicissitude of life, whether in delicate health, or in full bodily vigour, with the perfect enjoyment of every sense and performance of every function.

What the *immediate* state of the parts may be, that gives rise to squamous disorders, is, for similar reasons, equally uncertain. Several opinions on this subject have been maintained. Some assign the production of scales to deficient energy in the extreme circulation, by which, taking for granted the epidermis to be a secretion, it is supposed that the vessels of the cutis are unable to secrete healthy cuticle. Others refer it to chronic inflammation seated in the capillary vessels. (*Plvmbe.*)

Some recent physiologists (Mason Good, Wilkinson, and others) " conceive the fault to lie in a morbid state or secretion of the *rete mucosum*, which they say is sometimes too dry, or deficient in quantity; sometimes, perhaps, absent altogether; sometimes charged with a material that changes its natural colour; and sometimes loaded with an enormous abundance of a glutinous fluid, and occasionally combined with calcareous earth." My own observations lead me to the conclusion, that there is an altered action, most probably analogous to subacute inflammation of the vessels that secrete the cuticle, if it be a mere inorganic exudation, or nourish it, if it be endowed with languid vitality. But whatever the immediate action may be primarily, or in whatever texture it may exist, there is no doubt that it soon extends to the vessels of the cutis itself, and, in severe cases, produces pretty active inflammation, and even destruction of this texture.

Some of the older writers refer the origin of squamæ to indurated papulæ, or large elevations of the true skin, which, by pressure or distension, injure the texturə of the cuticle.

The structure of the scales produced in these disorders, it may be observed, seems to vary in proportion to the activity and intensity of the morbid action. In the slightest forms of *Pityriasis*, the scales consist of cuticle *alone*, which appears to be formed in exuberant quantity, and separated in branny desquamations. In lepra, or psoriasis, the exfoliations consist of several laminæ of diseased cuticle agglutinated together. They have an unctuous and adhesive feel, though very friable, a silky lustre, and considerable pearly opacity. The examination of the inner surface of one of these scales, by means of a convex glass, enables us to discover a minute protuberance, considerably shorter than its breadth, and which has evidently occupied a corresponding hollow, or excavation in the denuded surface; and if the scale has been separated by force, before the natural period of exfoliation, a speck of blood usually occupies this excavation; and the point described as the centre of the inner surface of the scale which occupied it, is also similarly discoloured. (*Plumbe.*) These observations, however, apply merely to Lepra.

Besides the projection just described, the under surface is found to be marked with lines or furrows that correspond to, or receive the lineal elevations of the subjacent cutis. If the scale has been long attached, and is hard, it may in consequence be so warped, that the reticulations, being in this way contracted, and rendered irregular, will not accord with the lines on the subjacent surface. In some cases, the scales accumulate so as to form thick and elevated white crusts, presenting an appearance not unlike a piece of asbestus.

But there is a third description of scales, such as characterizes those of ichthyosis, which may be defined horny verrucous elevations of the cuticle that overlap each other, like the tiles of a house, or the scales on the back of a perch.

Sometimes the squamous patches retain steadily the site that they first occupied; in other instances they disappear rather suddenly, and attack other parts under the same form. Sometimes these disorders creep as it were insidiously over the whole surface; hence the various appellations of *Serpigo*, *Herpes*, &c. that were applied to them by the older writers, and which, indeed, are not altogether discarded from modern nomenclature.

The disagreeable feelings produced by squamous affections, are tingling, itching, burning and soreness, which, in ulcerated cases, often amounts to absolute pain.

These sensations are more or less severe, according to circumstances; season, climate, certain kinds of diet, habits of life, state of the system, and the like, exert considerable influence in aggravating or assuaging the uneasiness they create; which, moreover, is sometimes not a little increased by the accumulation of scales and discharges that occasionally are secreted from the diseased surface. In this respect their site also is of considerable importance. "Thus, we find them extremely troublesome when seated between the nates, on the coccyx, or towards the verge of the anus; a part on which they are often met with, in persons who lead an habitually sedentary life. When situated between the nates, the unctuous secretion, that naturally lubricates the parts, becomes diseased, and contributes in a striking degree to their aggravation. When the eyelids or tarsus also are attacked, these diseases excite a sensation like the stinging of ants, that is extremely difficult to allay, and which the patient, by continually rubbing the parts, greatly increases." *

In short, it may be remarked, as a general rule, that the uneasy sensations which these disorders produce, are proportionally severe, as the parts which are attacked are endowed with more sensibility; hence, when seated on the face, the troublesome sensations produced by *cutaneous eruptions in general* are always most distressing. There is no doubt, however, that the constant exposure of this surface to the action of cold atmospheric air, often loaded with various pulverulent and irritating matters, contributes much to aggravate the patient's sufferings. In these cases, he often complains of such heat and burning, that he dare not approach a fire; and even the heat of his bed is productive of an increase of suffering.

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* Have not many writers on Syphilis mistaken these appearances for the proper symptoms of that disease?

On the testimony of Alibert, these complaints are not peculiar to man, but in some instances attack the lower animals, particularly the noses of cattle, especially the horse. What is called mange in dogs is often of this description.

Respecting the proximate cause of squamous complaints, it has already been stated, that nothing satisfactory has been ascertained. The exciting or occasional causes are various; but whatever they may be in any particular case, it appears to me, *that they would but rarely operate, did there not exist a great predisposition, peculiar idiosyncracy, or hereditary tendency to these complaints.*

Some varieties of squamous affections appear to be endemic. This is remarkably the case with *Pellagra*; but they are in no instance contagious, as has been erroneously imagined by Sennertus, Mercurialis, and many others of the older writers, who regarded them as such, from confounding them with the Hebrew or Arabian leprosy; the causes for which, and any analogy that exists between the two complaints, need not be here insisted on.

Respecting the possibility of their being hereditary, my own observation confirms me in the affirmative; I have met with some of them, especially psoriasis, so frequently in members of the same family. Mr Plumbe, under circumstances leading to the supposition that they are hereditary, refers their cause to certain uncleanly, idle habits, and kinds of life pursued by the victims of such disorders; and contends that, at all events, hereditary predisposition cannot be communicated beyond a certain dryness of the cuticle, dependent on the original formation of the cutis; and this disposition, he thinks, may be much aggravated by accidental circumstances. The explanation is by no means satisfactory. Nor have I always found the disposition connected with inattention to cleanliness.

Squamous diseases are divided, in modern nomenclature, into four genera.

Lepra, Psoriasis, Pityriasis, and Ichthyosis.—The two first of these appear to differ from each other, simply in the accidental form which the eruption assumes, and do not afford sufficient ground to admit of their being regarded as distinct genera, notwithstanding the authority derived from their having been considered as such from the days of Hippocrates downwards. Willan has adopted this division, and to dissent from him may be considered presumptuous; yet the experience I have lately had in the treatment of these complaints, cannot in this instance be made to coincide with his authority, and compels me, though not without diffidence, to recede from his views of arrangement.

Although the foregoing prefatory remarks apply to squamous disorders in general, yet it is my intention in the present Essay to confine my attention to the two first of Willan's species, Lepra and Psoriasis.

Of Lepra and Psoriasis.—Both these terms are somewhat objectionable; the first from its having been erroneously applied to denote two diseases of very different natures, viz. the *Lepra Arabum*, or *Elephantiasis*, and the *Leuce* or *Hebrew Leprosy*, as well as the Leprosy of the Greeks, to which it is now limited by British practitioners; the latter from its primitive *Psora*, with equal indiscrimination having been used by the Greeks, to express a particular disease, now denominated *Impetigo*, as well as *Scabies*, or *common* itch. To reconcile these inaccuracies, the term *Psoriasis* was adopted by Willan to denote the scaly disease in question, and has since his time been pretty generally employed, and may be now said to be in common use.

With this explanation, I shall proceed to treat of Lepra and Psoriasis under the single appellation of *Lepra*, not because I conceive it the least ambiguous of the two terms, but as it appears in most instances to be the primary form, from whence Psoriasis and its varieties proceed; and the old expression is retained, as I am not prepared to offer one less faulty; besides, the meaning attached to the term *Lepra* is now very generally understood, at least by British practitioners, and any innovation of the title might, instead of contributing to the elucidation of the nature of the disease, tend to involve the subject in still further obscurity. Lepra then * "is characterized by scaly patches of different sizes, often exhibiting a circular form;" the borders of the patches are somewhat elevated above the centre, and more scaly than it, particularly in the advanced stages of the disease, at which period the centre is often nearly healthy skin, the patches, as in many other cutaneous affections, healing from the centre towards the circumference.

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* Dartre furfuracée arondie of Alibert. 12th Plate of his large work, 3rd volume.

It usually commences with small elevated papular spots, of a rounded form, and varying in size from that of a divided coriander-seed, to the dimensions of a split pea, or larger; in children they are often much smaller than here stated.

Their surface is sometimes rough, but more generally smooth and polished, and not marked with cuticular lines. These elevations are highly vascular, and, if cut into, bleed, but do not contain any fluid. In persons whose skin is delicate, or in children, they do sometimes, if rapidly formed, contain a small vesicle in their centre, but its contents generally dry up without bursting. In the course of a day or two, sometimes in the space of few hours, a small semitransparent nearly circular scale may be observed upon the top of each spot, which soon exfoliates, or is rubbed off by the friction of the dress; in either case, it is speedily reproduced. At first it is about the thickness of fine paper, and not unlike the scale of carp, but if allowed to remain undisturbed, it becomes laminated, and increased in density and opacity.

When the primary papular spots are pretty thickly distributed over the surface, and not very elevated, or perhaps even a little depressed in the centre, the part exhibits an appearance as if dried herring scales were pasted on the skin, here and there alternating with shining red spots or stains, on which scales may not have formed, or from which they may have been abraded; thus presenting a speckled appearance of red papulæ, patches, and silvery spangles, dispersed irregularly over the part, and in some places clustered or running together. In this state it is most frequently met with in children and young girls, till the age of fourteen, and is denominated by Willan under this form, Lepra-*Alphoides.* *

In the eruption, as it occurs in adults also, the site of the scales is always considerably redder than the surrounding parts, and when exfoliation spontaneously takes place, presents a glistening surface, sometimes smooth, but in chronic cases, wrinkled, and irregular, or intersected with fissures, corresponding in general to the under surface of the desquamated layers; which it has been observed, "do not correspond with the lines of the contiguous sound cuticle" (*Willan*). The explanation of this, however, was sufficiently given in the introductory remarks, (page 4),

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* This epithet he appears to have adopted, and applied *determinately to this accidental form of Lepra,* in order to extricate himself from the perplexing contradictions that are met with in the ancient writers respecting the proper application of the term *Alphos;* and not from any conviction in his own mind that there was actually such a difference between this and his first variety, Lepra *vulgaris,* as to constitute a decided species. This conclusion I think will be sufficiently evident to any one who peruses Willan's description of Lepra Alphos.

The appearance presented upon the exfoliation of the morbid cuticle, it is obvious, must vary considerably, as the disease may be of longer or shorter duration, and the length of time that the scale has taken to become detached. If forcibly removed before the exfoliating process is pretty nearly complete, the true skin may be lacerated and bleed, but if the scale is only slightly attached, it may be removed with impunity. This, though a simple, and apparently easily explained phenomenon, which could scarcely escape either the observation of any one, or meet with an erroneous interpretation, has been particularly noticed by Mr Plumbe, as a peculiarity of the *first formed* or *mother* scale, and not possessed by any of succeeding growth. This central adherence does not appear to me to be in the least remarkable, and can easily be explained, by supposing that the exfoliating process commences at the circumference, and verges to the centre, which, being the last part of the scale freed from its attachments, will hence lacerate the subjacent cutis, and produce a speck of blood, if prematurely and forcibly detached. But I cannot say, although I have directed particular attention to the investigation of this point, that I have been able to satisfy myself of the accuracy of Mr Plumbe's opinion.

But to return from this digression; in three or four days the small elevations first described seem somewhat flattened, and at the same time dilated by an extension of their base in every direction, so as still to retain their primary form, and attain the size of a sixpence, and they afterwards gradually, or perhaps rapidly, increase to that of a crown-piece, or much larger. These patches are covered with white silky scales, and are surrounded by a slightly inflamed border, to the extent of a few lines. In some cases, the accumulation of morbid cuticle is such as to form a thick prominent white crust, and, when detached, is soft, unctuous, and easily pulverized. If the patches are distinct, not very inflamed round their margin, and somewhat angular in shape, not depressed in the centre, and not larger than a sixpence, then the disease bears all the characters of the first variety of Willan's psoriasis, and is by him denominated Psoriasis *Guttata.*

In this form the disease is very apt to occur as a syphilitic affection, in which case the scaliness is not in general so great, and the patches exhibit something of a brick-dust red, or copper hue; occasionally the colour verges to purple.

Why the patches should retain, in their extension, the ovoid or circular form, which is not, by the by, peculiar to lepra, but common to many other diseases of the skin, has to many appeared singular. Some refer it to a peculiar disposition of the vessels of the corion, which they imagine to be disposed in concentric circles; but it appears to me that the best solution of the fact is, to explain it upon the principle of extension by continuity of surface. Thus, if a drop of oil be allowed to fall on a piece of blotting paper, its stain spreads in the original orbicular form; or if a stone, whatever its figure, be thrown into a pool of water, the first impression extends by circular ripples, from the impetus communicated on every side to the displaced fluid. As the circles become very large, they gradually lose the effect of the primary shock, and fall into irregular shapes, or disjointed undulations. In the same way, then, it appears to me that the round patches, considered characteristic of lepra, extend in every direction, and the circular form is retained, until the circumference is either so far removed from the primary morbid action, as not to be completely within its influence, or until, by the coalition of the extending patches, the original figure is lost, and an irregular one is produced; and in this state, as far as I can see, the disease differs in no essential particular from what is denominated Psoriasis *diffusa*, which, even according to Willan's own view, differs from lepra only in the form and distribution of its patches, and in its cessation and recurrence at certain seasons of the year, and by the disorder of the constitution, with which it is often attended. But in the first of these characters, it is not by any means particular, and the last is merely the consequence of a more severe and aggravated disease.

Paulus Ægineta, in a comparative view of lepra and psora, by which latter term he denominated psoriasis, states, that the *former* affects the skin deeply in patches of a circular form, and throws off scales like those of large fishes, whereas the latter is more superficial, and variously figured, and throws off bran-like substances. But the possibility of making this diagnosis, is far from being constant in any series of cases. Indeed, in most instances, the two states exist at the same time on different parts of the body.

But it does not necessarily follow that the above is always the course of the disease; sometimes there is an irregular, reddened, rough, choppy state of the skin, thinly interspersed with scales, and deep furrows resembling much the chopping produced upon the arms by cold, which, with other similar variations, is also a modification of the same affection.

If, instead of coalescing, the patches are sufficiently far asunder to retain their primitive circular figure, after a time the central portion, that first formed, beginning to heal, becomes somewhat flatter than the circumference, which latter, in consequence, appears a little elevated; and as the edges extend, the included area gradually becomes sound, or retains for a length of time a harsh feel and reddish brown hue; the external edges are generally in this stage a good deal inflamed. This state constitutes the true *lepra vulgaris*.

In cachectic habits of body, the inflammatory areola, instead of being of carmine red, is often of a purplish hue; and the surface underneath the patches, and even the scabs themselves, assume the same tint. The latter, in such cases, are generally very thin, and are more easily detached than in the other forms of lepra; and owing to the unhealthy state of the general system, the surface sometimes becomes excoriated, and discharges a mixture of lymph and blood, till a new incrustation forms, which is hard, brittle, and irregular. The disease, under these circumstances, is denominated *lepra nigricans.* * It is very apt to run into ulceration.

A variety of *psoriatic lepra*, that is described by Willan, under the title of *P. gyrata*, is sometimes an accidental appearance arising from the extension or healing of lepra by one of the very various modes in which this may take place. In this, considerable portions, or segments of the circular patches, successively heal in such a manner, that the remaining parts form a continuous, but tortuous line. Often, however, it is an original formation, and is distributed in narrow patches or stripes, variously figured; some of them are nearly longitudinal, some semicircular, vermiform, tortuous or serpentine. They are very thinly covered with scales, the diseased parts presenting little more than a brownish, harsh, and sometimes fissured surface, not unlike the roughness that is produced on the skin in very cold and frosty weather.

Several *local* varieties of psoriasic lepra are enumerated by modern authors.

Such is the disease, as it is frequently met with on the *palms* and *backs of the hands* of certain artificers, in consequence of the application of certain acrid substances; that of the *eyelids*, angles of the mouth, and borders of the lips, and a troublesome scaliness of the scrotum, prepuce, and edges of the skin surrounding the nails. But in these we very rarely find it a perfect squamous disease; it is more frequently merely a roughened, harsh, chopped, or fissured state of the parts, accompanied with a shelling of the cuticle in thickened, circular, flaky desquamations, which do not present the laminated white and silky character, or attachment of the true scale; yet true scales, something like those of a herring, thin and shining, are here and there thinly interspersed over the diseased surface.

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* I have seen a slighter degree of this nigrescent appearance, on the decline of the lepra vulgaris, apparently from no other cause than the change of colour incidental on the diminished energy of the inflammation. In the venereal disease, circular patches sometimes appear, which resemble those of lepra nigricans in size and colour, but it is generally an exfoliating, and not a scaly state of the cuticle, that covers them.

This state is, moreover, very seldom met with uncomplicated with other diseases, of which it is in general to be regarded a degeneration. Thus the *psoriasis palmaris*, as it is called, so commonly met with amongst bakers, bricklayers, printers, &c. from the irritation produced by the constant application of flour, brick-dust, and printers ink, commences for the most part by pustulation, and the formation of vesicles, constituting in this stage a local affection compounded of *Eczema* and *Impetigo*, and known by the compound name of *Eczema impetigonodes.* It is therefore accompanied, particularly in its early stages, with a certain degree of discharge, and hence named *Humid Psoriasis*, or *Tetter.*

A diffuse scaliness, that occurs during infancy, attended with great itching and inflammation, partakes very much of the humid character, and often presents an appearance as if the skin were excoriated, in patches of very considerable extent, over which is scattered a number of carp scales, or small pieces of very finely split talc.

It is further worthy of notice, that this *infantile* psoriasis is often accompanied with pustulation, obstruction of the nostrils, "occasioned by a morbid secretion, and scabs formed internally;" and if the face or forehead is affected, the eyelashes and hairs of the eyebrows fall off.

When *Psoriatic Lepra* has existed for a great length of time, and cleanliness is neglected, it often becomes a very severe and distressing complaint; the skin acquires an almost universal scaliness, and becomes harsh, furrowed, dry and indurated; the primary patches coalesce, or become confluent, so as to spread at length over a very considerable portion of the body; and painful rhagades are interspersed, wherever, from the motion of the joints, or any other sudden dilacerating force, the unyielding parts offer least resistance to rupture.

The production of scales is so rapid, that large quantities of them are found on the bed on which a person afflicted with this disease has slept. They fall off in the same proportion during the day, and being confined by the linen, excite a troublesome and perpetual itching. The nails of the fingers and toes become convex, and indented with longitudinal furrows and more thickened at their extremities; curdy fetid matter is deposited at their roots, and a frequent renewal of them takes place, the new nails soon assuming the morbid form. The thumb and fingers are enlarged and contracted at their articulations, or in some cases retorted. On the abdomen the skin is very red, deeply indented and brittle; no thick incrustation forms here, but the scales appear thin and semitransparent, peeling off from time to time in large flakes. Painful excoriations are occasioned by the pressure of some parts of the clothing, or by the attrition of contiguous surfaces, as of the nates, groin, thighs and scrotum, from whence a very profuse discharge of lymph takes place. In this irritable state, it is not uncommon to find interspersed over the parts, hard, painful, indolently suppurating boils, or ecthymatous pustules, or to see the whole scaliness disappear, and leave these acutely sensible semitubercular phlegmons, intersected with deep fissures, and excoriations, which bleed upon the slightest motion of the part; and in this state the disease may continue without any material remission for a considerable length of time, especially in old people.—This also is a variety of *Psoriasis Inveterata.*

On the decline of the eruption, in whatever form it may have existed, the scales, roughness, rhagades, or induration of the parts, gradually disappear, and a new cuticle is formed, at first red, dry and shrivelled, but which, in two or three weeks, acquires the proper texture.

Having now offered a general delineation of lepra, and its psoriatic varieties, in so far at least as meets their *external* appearance, we shall proceed to consider,—1*st*, On what parts they are most liable to occur; 2*nd*, The troublesome sensations, or painful feelings they excite; 3*rd*, The constitutional symptoms; and *lastly*, Their causes, and medical treatment.

I. *Of the parts which they attack.*

The knees, immediately below the patella, and over the course of the tibia, for about one third down the leg, the elbows, outer part of the fore-arms, and along the surface of the ulna, are the parts on which leprous spots generally first show themselve. Within a few weeks, however, the primary spots coalesce, and spread in various directions, but generally appear to select those parts wherein the bones or aponeuroses lie near the surface, so that the integuments are rendered more tense, and where it may be presumed that the supply of blood is not so perfect, but liable to interruption from the motions of the parts, or the accidental external application of pressure. But as the disease advances, it spreads over parts *indiscriminately;* so that by the coalition of the borders of the extending scaly patches, the whole of the softer and more muscular parts, both of the leg and thigh, arm and fore-arm, are gradually involved. When it attacks the *back* or *chest*, the integuments seated over the course of the spine, the spine of the scapula, the ilium and sacrum, in the back, and those along the course of the sternum, in the breast, are the parts first affected. Often there is a large patch seated over the lower part of the abdomen, extending to the pubes; and again, on those parts of the groins where the fasciæ are most abundant and strongest. The head is usually the last part affected, but I have frequently seen it the first, and not only so, but the only part of the body on which the disease occurred. When seated on the scalp, it forms thick incrustations among the hair, and is with difficulty distinguished from *Porrigo* or *Tinea Furfurans.*

The patches in this situation retain long their circular form before they coalesce; and lepra as most eruptive complaints, when affecting the cranial integuments, derives some peculiarity from their structure. There is often a severe itching, and exudation of ichorous discharge, accompanying the appearance of scales about the roots of the hair; in fact, the complaint in this situation is more frequently complicated with others. Any peculiarity that cutaneous diseases present when seated on the scalp, Mr Plumbe, with much appearance of truth, refers to the irritation produced by the passage of the hairs through the part.

Now, although fresh patches arise from time to time in different situations, there is no alteration in the state of the parts first affected, as happens in some other cutaneous affections; the scaliness still continues to fall off, and recur for an indefinite period; but when the complaint is about to terminate, the scaliness ceases in general to appear first in the centre of each patch, and extends to the circumference; but this is not an invariable rule, as may have been inferred from the observations offered a few pages back, on the mode of extension of lepra.

The face, comparatively speaking, is seldom affected, and the scales which form upon it when it is the seat of the disease, are very delicate and transparent, and easily separated by washing; indeed, from the daily practice of ablution alone of this part, it is evident they must be exposed to constant abrasion, and are not allowed to acquire any degree of thickness.

The degree *of depth* to which the surface is diseased, varies in different cases, and will of course depend very much upon the severity and duration of the complaint. In milder examples of the disease, the morbid action appears to be confined completely to the scarf-skin and vessels constituting the tissue placed over the corion; but in more severe and old standing cases, the true skin and subjacent parts are involved.

We shall now, in prosecution of the order above premised, proceed to investigate the troublesome or painful sensations that accompany the different varieties of leprous affections.

In the mildest examples where the eruption is not very extensive, the exfoliation slight, and not strongly attached, the patient seldom experiences any inconvenience, unless, perhaps, a slight itching or uneasiness after a full and indigestible meal, or during particular states of the atmosphere; an observation that attracted the notice of the Father of medicine, who tells us that "some Lepræ itch before rain." These feelings are also augmented on becoming warm in bed, when the body is unusually heated by exercise, deranged by indulgence in fermented or spirituous liquors, or by any other excesses or irregularities of life.

These sensations, though comparatively trifling, and perhaps not deserving of any other appellation than that of *uneasy feelings*, are in many instances far more insufferable than actual pain. Hence Galen observes, so intolerable is the itching, or formication in patients prone to irregularities, that they are often compelled to pass the night in restlessness; or, if occasional sleep is obtained, they start from it, and as it were involuntarily leap from their bed to obtain relief, so intolerable and tormenting are the sufferings induced by the itching.

In extreme cases, where there is considerable disease of texture, accompanied by active symptoms and some slight discharge, the formication amounts to a sense of soreness, burning heat, or even pain, and there is considerable stiffness of the parts, which impedes or restrains the motions of the limbs; so that all these circumstances combined, in severe cases sometimes oblige the patient to keep his bed.

Mr Plumbe, in treating of the uneasy feelings that accompany leprous diseases, remarks, that there is one circumstance peculiar to them, which has been often particularly mentioned to him by the patients, viz. a sensation of pricking, most frequently noticed a little before the separation of the *first* scale, when perhaps, it has hardly attained the size of a spangle. He is inclined to think this sensation the consequence of the raising up of the edges of the scale, produced by the tumefaction and elevation of the inflamed margin, and fresh growth of scale, the centre which is adherent to the cutis being thus forcibly torn from such attachment. This conjecture, he argues, is rendered more probable by the fact, that when the disease is obviously subsiding, when no new scales rise up, and thrust those which were before formed from their attachments, the pricking in question is no longer felt. (*Plumbe*, p. 286.) The result of my own experience does not tend to confirm this ingenious theory.

We now proceed to treat of the constitutional symptoms of lepra, and its various modifications.

It is somewhat singular, as was remarked in the preliminary general observations upon Squamous Disorders, that a disease, such as we have been describing, wherein it is probable that one of the most important functions of the body is impeded, can exist, even in its slightest form, without proportional inconvenience and derangement of the general system; yet such is the fact; for as was before observed, persons affected with lepra are generally the most healthy. Occasionally, however, constitutional symptoms do both precede and accompany, *or are produced by this disorder*, in its more severe or psoriatic forms. It may here be remarked, that Willan enumerates disorder of the general health among the diagnostic marks that characterize psoriasis from lepra. But it does not appear to me that this diagnosis is worthy of confidence,—it can at best be considered but a proof of greater severity in either form,—and does not in the least alter the local peculiarities by which the disease is known; and this is a disease which is alone to be detected by local characters.

The constitutional symptoms of squamous, as well as of all other cutaneous affections, ought to be considered in two points of view.

1*st*, As producing the local disease:

2*d*, As produced by it.

In other words, they ought to be treated of as of *primary* and *consecutive* origin.

The general primary symptoms are for the most part such as arise from dyspepsia, or irritation of the primæ viæ, viz. headach, inappetence, pain or sickness at stomach, occasional vomiting, cramp of the bowels and extremities, coldness of the latter, and a universal sense of languor and debility, which are accompanied, in some instances, by ephemera and its concomitants. If these have been induced by some offensive article of diet (one of the most common occasional causes of these disorders), they will, in all probability, be mitigated, or they may wholly disappear on the accession of the eruption. If they are the consequence of subacute inflammation, or other chronic disorder of an internal part, they will, it is obvious, accompany the disease in all its stages, unless removed by suitable remedies.

But the most unmanageable form of general constitutional distress, is that which results from the irritation produced by extensive and severe disease of texture, in which case, symptomatic fever may be expected, or from the derangement occasioned in the functions of the skin, which in their turn prove a source of disturbance by sympathy, or by direct determination to internal organs. Thus we may have inflammation, or congestion in the lungs, brain, or other internal parts; hectic fever, dropsy, or marasmus. "Multiplicatâ materiâ, obstruantur lymphaticæ glandulæ; cachecticique tandem atque hydropici moriantur, aut tabe pulmonali, absumantur ægrotantes." (*Lorry*, 367.)

But there is still another complication, which, although it cannot render the case much worse than just stated, yet it deserves notice; for we have hitherto been considering the primary and consecutive symptoms as existing separately. It is when the complaint arises from chronic internal disorder, either of structure or function of any particular part; and to the symptoms thus induced, are superadded such as arise from the local irritation externally produced; the case then presents a most inauspicious appearance, and often, under such circumstances, baffles medical aid.

Examples of this description are very rarely met with in this country.

In cases that arise simply from disturbed functions of the *stomach and primæ viæ*, such as are said to occur particularly during child-bed, when these organs are more easily deranged, the morbid accumulation of cuticle is comparatively trifling, and seldom so great as to become a source of irritation sufficient to produce reaction upon the general system.

Causes.—Respecting the *predisposing* and *proximate* causes of *lepra*, little or nothing satisfactory can be sad; the disease occurs occasionally in every variety of constitution, and under every circumstance of life; but as was before observed, the stout and plethoric are the most liable to it. The greater number of cases that apply at the Dispensary are ruddy, fair-complexioned persons, who evince no symptoms of a "*languid state of the circulation,*" quite the contrary. Indeed, I cannot enter into the notion, that *deficient energy* of the *circulation* can predispose to any disease, the proximate cause of which appears to be *excitement* or *inflammation*; yet this is the common doctrine.

It appears to me that this is a kind of paradox, which has unconsciously, on the part of its promulgators, crept into the pathology of squamous diseases, and has materially contributed to impede the improvement of their treatment. Thus Willan, at the 125th page of his quarto work, in treating of Lepra, observes, " A slow pulse, or a *languid circulation* of the blood, and, what must generally be connected with it, a harsh, dry, impermeable state of the skin and cuticle, appear to constitute a fundamental part of the predisposition."

And again at the 175th page, in stating the habit of body in which psoriasis is most liable to occur, he says, " In such persons, the pulse is *feeble and languid, or irregular*." But at page 112, he tells us that " leprous spots are surrounded by a *red* border;" which we are led to conclude he regards to be a redness produced by inflammation, since he afterwards characterizes the patches of *Psoriasis Guttata* as differing from *Lepra*, " in having *little or no inflammation* round them" (p. 152). Bateman, who generally follows Willan, expresses himself in similar terms. At page 28, he speaks of " a considerable degree of *inflammation* of the skin;" at page 32, he alludes also to cutaneous *excitement* and *inflammatory* action; but at page 38, he tells us, that those who have a dry skin, and *languid circulation*, are most liable to these diseases. Mr Plumbe also falls into the same apparent paradox, and more completely so than either Willan or Bateman; at page 289, he treats of " the *inflammatory action* which constitutes lepra;" and most of us admit inflammation to consist in an increased action of the sanguiferous system; but at page 294, he certainly contradicts himself, when he states, that " deficient energy in the vessels of the skin, and consequent inadequacy to the production of healthy cuticle, may be fairly presumed to exist in this disease." On the one hand, therefore, he considers the disease to be constituted of an *inflammatory action*, while, on the other, he denies that the vessels of the part have even their natural energy.

Now, what I am desirous of proving is, that the commonly assigned *predisposing* and *proximate* causes of the complaint are inconsistent with each other. We never think of referring pneumonia, or any other inflammation, or, in fact, any disease where there is acknowledged vascular excitement, either general or local, to habitual want of vascular energy, or a " languid state of the circulation," as Willan and Bateman have done; nor when the inflammation is actually in existence, has it yet been fully established that its proximate cause is *a deficient energy in the vessels of the part, as Mr Plumbe supposes.*

My own opinion is, that the predisposing cause is not at all known, since the complaint can, and does arise in persons of very opposite temperaments; but, if I may credit daily experience, more especially in the sanguine, wherein *a languid circulation, either local* or *general*, cannot even be suspected. I imagine that the primary evil lies in the secretions of the true skin, which, becoming vitiated, by their local irritation induce chronic or subacute inflammation of the vessels, that either nourish or produce the cuticle; and that, instead of not being able "to secrete a *due proportion of healthy cuticle*," as has been expressed, they *produce a superabundant supply of morbid cuticle.* Have we not stated amongst the symptoms a fact that is known to every one, that so *rapid is the production of the scales*, that, in the course of a night, the sheets of the bed will be literally covered with them? How then can we talk of the vessels being unequal to produce an adequate supply of cuticle? The scales, we have shown, are nothing but morbid epidermis.

The secretions of the surface may be very easily deranged by acrid ingesta, in persons predisposed to cutaneous diseases; and we know that, when once a secretion is vitiated, the natural action which produces it is not speedily restored; of this we have examples in *Gleet*, *Diabetes*, *Catarrh*, and many other diseases of secreting organs.

The validity of this doctrine will be more particularly pointed out when we come to treat of the *modus operandi* of such remedies as are satisfactorily known to remove the complaint.

The occasional or exciting causes of this disease are much better understood, though by no means in all cases obvious. Nor do those which may be pointed out with some degree of certainty, seem to possess a peculiar power to produce the squamous, in preference to any other class of cutaneous maladies. Indeed I am of opinion, that there is strong reason to believe that the external form of cutaneous disorders, in general, is in a great measure incidental; and that an agent which would produce lepra in one person, might induce *Impetigo*, *Nettlerash*, *Lichen*, or *Herpes*, in a second, and *general constitutional febrile derangement*, without perhaps any *external* affection, in a third. Sometimes we find that these complaints are actually produced first, and that a squamous, or some other affection, occurs as a sequel. Thus, it was known to the ancients that lichen, especially after frequent returns, is liable to become psoriasis.

To me, then, it appears, that in very many instances the fault lies primarily in the vitiation of the cutaneous secretions, which may be *variously* diseased, by different causes, in different individuals; and hence the multiform appearances that skin complaints assume, though it does not follow that each kind of derangement of the secretions will necessarily produce its own specific external disease. Mr Wilkinson of London, on the contrary, who has written a practical little pamphlet on this subject, states it as his belief, "that the diseased state of the secretions *is always the same*, and that the varieties of the eruptions are produced solely by the conformation and constitution of the subject.

"We continually observe," says he, "that the same causes produce very different effects in different individuals; that the same medicine vomits one person and purges another, and excites a diaphoresis in a third; that the cold damp air, acting upon two people at the same moment, in the same situation, produces in the one a pleurisy, in the other a typhus fever.

"These circumstances are familiar to all of us; and we easily explain them, by considering the different habits, and the different states of system, of those who are, at the same moment, exposed to the same causes. Yet, though equally applicable, we seem to have neglected these observations when considering the morbid phenomena of the skin; for the same cause, exciting the same defective or deranged cuticular secretion, may produce the different varieties of lepra in one person, of psoriasis in another, and of pityriasis in a third."—*Wilkinson, p.* 2.

Whether, however, the occasional or exciting causes operate through the medium of the secretions or not, we cannot with certainty determine; they are, at all events, such as there is a probability might produce in some cases their *primary*, and in others their *secondary* influence in this manner. Thus, exposure to cold and moisture, accumulation of sordes on the skin, the application of local irritants, particular articles of diet, such as dried fish, oatmeal, and some incongruous mixtures of food, enumerated as the most common exciting causes of lepra, are perfectly capable of operating, some *directly* on the skin, and others *indirectly* on this part, through the medium of the digestive organs. Thus, from the first set of causes, we find that the disease attacks sailors, scullermen, brick-layers, coal-heavers, watchmen, dustmen, working chemists, bakers, grocers, washerwomen, and particular artisans; and from the latter, individuals of peculiar idiosyncracy, or persons in whom there is a remarkable natural proclivity to cutaneous disease, either from hereditary predisposition, or some other unaccountable conformation. Willan admits that, even when it arises in consequence of local irritation, as in bakers, or brick-layers, &c. that it is not to be supposed from this to be general amongst those classes of persons; but that the complaint is merely aggravated by their business, and that it does not affect them unless otherwise disposed to it; a mode of reasoning rendered more than probably correct, from the following facts. "1*st*, The disease, even in these persons, disappears about midsummer, and returns during the cold weather at the beginning of the year. 2*d*, Persons constantly engaged in their business, after having been once affected with the eruption, sometimes enjoy a respite from it for three or four years; and, 3*d*, When the business is discontinued, the complaint does not immediately cease."

Bateman informs us that he has known the disease produced by hot and spiced soup, by copious draughts of cream, by the use of vinegar, and other articles of a similar nature. Some poisonous substances taken into the stomach, are reported to have produced Lepra; thus, in the 3d Vol. of Med. Facts and Observations, the poison of copper is stated to have speedily excited in several persons at the same time an eruption of this character, which in one of the cases continued for a month, in the others it disappeared in about ten days. According to Willan's experience, it is the *psoriatic* forms of lepra, that are most commonly produced in this way. *True* lepra, he thinks, "can seldom be referred to diet," or irritating ingesta.

Of the effect of syphilis in producing this and other scaly eruptions, and the modifications effected on their common characters by the venereal virus, I shall not at present offer any comments, as it appears to me that this subject would be better considered in a separate essay. I shall now, therefore, proceed to treat of the *cure* of lepra.

Treatment.—The flattering success that has attended the treatment of leprous diseases in the practice of the Institution in Niddry Street, enables me to speak with great confidence on this division of the subject. But, in order that my readers may have a comprehensive knowledge of the plans that have been recommended by other practitioners, I shall take a general survey of the different remedies that have at various times enjoyed either deservedly or otherwise their share of approbation, and afterwards state as briefly as possible the system usually adopted by myself. Not that it differs materially from that of other practitioners, but it is perhaps somewhat differently applied.

The Greek physicians trusted principally to external applications, such as alum, sulphur, nitre, elm bark, goats-dung, viper's flesh, urine, hellebore, colophony, mustard, horseraddish, quicklime, vitriol, and the like; but these remedies, as might be expected, were either wholly useless or productive but of temporary relief. Some premised bleeding, but it was only to a trifling extent, and unless aided by *internal* remedies, in most cases, could be of little avail, in many it would be obviously improper.

Liniments composed of tar, or of some of the various mercurial preparations; turpentine, lime water, aqua potassæ, blisters, decoctions of bran, elm and oak-bark, and many other such remedies constitute the more *modern* topical applications; but they are equally ineffectual in the majority of cases, without the aid of general treatment. As remedies that operate either *directly* upon the diseased part or through the *medium of the constitution*, may be enumerated friction, warm bathing, whether in a slipper-bath, the sea, or in any of the medicinal waters of Harrowgate, Moffat, Croft, Buxton, or Bath. The sulphureous springs more especially have deservedly acquired celebrity, both as internal and external remedies. To these may be added, the ancient, simple, and medicated vapour-baths, that have within the last few years been revived both on the Continent and in Britain. They are of infinite service as auxiliaries, and may be employed with much more freedom than is commonly believed. Combined with moderate friction, they contribute to remove the scales, and to produce a soft skin, which soon regains the usual colour and texture. They are by many regarded as sufficient for the cure of the slighter cases of lepra, without the use of internal remedies.

Bathing the whole body may not be requisite if the eruption be very limited in extent, unless seated on parts where topical bathing cannot be easily applied. Fomentations may often be substituted.

Among the internal remedies, may be enumerated sulphur, antimonials, and nitre; the various decoctions of sarsaparilla, mezereon, guaiacum, dulcamara, and elm-bark. The first three are said to be of little or no avail, and the latter have very improperly by some been ranked as specifics. To these may be added, the different mineral acids, alkalis, &c.; and finally, mercurial preparations with a view to produce salivation. If any of the latter are deserving attention, it is the murias hydrargyri dissolved in alkohol. It is often a useful external application, and given internally in small doses, it certainly does frequently produce a decided improvement in the state of the surface. More lately arsenic has been introduced, and it is perhaps of all other internal remedies the most to be depended upon in the cure of leprous affections, and more than any entitled to be considered as a specific, but its use requires judicious management, and is not proper in every state of the complaint. Indeed, here lies a radical defect in all the treatises

I have read on the cure of Lepra. Remedies in abundance, and powerful ones too, are enumerated, but the indications for their employment are never precisely explained, so that we are left perfectly in the dark respecting the states of the disease in which they are, or are not proper.

Hence, the most powerful and deservedly extolled medicines, administered without judgment, and at improper stages of a complaint, prove injurious in their effects, and their true value is undeservedly depreciated.

It must have been evident, from various observations that were made in treating of the history of these diseases, that there are two states in which they can exist, viz. in an *active, irritable, or inflamed* state, and in a *chronic* or *inactive* one. And it is upon this that we found our mode of treatment, *and not upon the name of the accidental variety of the complaint*, as is too commonly the case.

When a patient presents himself, we investigate, 1*st*, The extent and duration of the eruption. 2*d*, Whether it has arisen from any cognisable cause. 3*d*, If the cause or its effects be still in operation. 4*th*, Whether the constitutional symptoms, if there be any, are *primary* or *secondary*, the meaning of which has been already explained: And, *lastly*, Whether the disorder be in an active or a chronic state; and of this we judge by the appearance of the eruption, the uneasy sensations, the irritability of the parts, and the existence or non-existence of fever.

When the eruption is limited in extent, has existed for a considerable length of time, and is unattended by either uneasiness, itching, pain, or constitutional disorder, we have reason to regard it, whatever it may primarily have been, as a disease simply of the texture of the part; and, in this case, local applications, such as have been enumerated, and topical bathing, comprise all that is requisite.

As a local remedy, the pitch, and weak nitrate of mercury ointments, are what are most generally prescribed, either separately or conjointly; or a solut. of the muriat. hydrag. corros. in dilute alkohol, and immersion of the part in warm artificial Harrowgate water, for the preparation of which, the following is the formula:—Sulphat. magnes. ʒij. Supertart. potass. gr. x. Sulphat. potass. cum sulphur.* ʒss. This quantity is directed to be dissolved in about twenty-four ounces of warm water, and the diseased part is to be washed with, or immersed in the solution, at about 95° F. of temperature, two or three times daily, for about fifteen or twenty minutes each time, and a mixture, of equal parts, of the weak citrine and tar ointments, to be applied after each immersion.

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* Formerly called Sal Polychrest.

Now, these applications, I conceive, operate by changing the secretions of the part, which undoubtedly they may effect by their stimulating properties; yet did their efficacy consist in these alone, other stimulants ought to answer equally well, while experience convinces us that they do not.

Supposing that the morbid part (still limited in extent) had been in an irritable and inflamed state, instead of that represented; in lieu of the above application, then, simple and repeated warm bathing, or ablution, the application of a good number of leeches, and a poultice till this irritable state should be subdued, would have been premised.

If we find the eruption *pretty extensive*, but in an *inactive* state, by which is to be understood that it is devoid of troublesome sensations, redness round its circumference, and constitutional symptoms; in that state, indeed, which may be denominated an adopted morbid action of the cutaneous structure, or to which it has accommodated itself in the same manner as non-secreting parts do to secretions, they are enabled to form during the primary stages of a disease with which they may be attacked. In that case, the treatment consists not only of external applications, such as are recommended above, but of warm bathing, and such remedies, as may be denominated *specifics*; and of these there are only two that are known, the murias hydrargyri and arsenic. Of their administration we shall immediately speak, after a few words further on the choice of local remedies and bathing.

As a topical remedy in *chronic* cases, we hardly ever employ but one ointment, viz. that mentioned a moment ago, a mixture of equal parts of weak nitrate of mercury and tar ointment. This is directed to be rubbed upon the part night and morning, and cleaned off with a dilute mixture of aqua potass. about ʒij to ℥vi of water, or with a little fresh butter or oil, as it adheres so strongly to the part that something of this sort is requisite, soap and water having comparatively little effect. On its removal, the diseased parts are to be well brushed with a solut. of mur. hyd. in proof spirit, in the proportion of about two grains of the salt to an ounce of spirit. Where the skin is naturally delicate, the above unctuous application is diluted with simple cerate, in one, two, or three proportions; and the spirituous solution is used not quite so strong.

Now, it may be observed, that these local remedies are always preferred, because others have been frequently tried, but found not to answer so well.

However extensively the above ointment is applied, and it is frequently rubbed over a very considerable surface at one time daily for weeks, I have never yet seen it affect the mouth, or produce any other of the effects peculiar to mercury. It operates, perhaps, by changing the actions of the parts; but there is a more decided mode of explaining its beneficial effects, which is this,—the tenacity with which it adheres renders it with difficulty removed, and the patient being directed to clean it away *thoroughly* prior to each new application, thus unconsciously submits the parts to a very perfect ablution, and considerable friction, two agents of the utility of which no person can doubt in the cure of lepra. The muriate solution sometimes affects the mouth.

Respecting the use of warm bathing, it may be observed, that in cases such as we are treating of, a bath of the temperature of 86° or 90° F. will always be useful, and it may in general be administered daily for weeks, without in the least injuring the general health; a fear that has crept into common doctrines, and which, I am certain, has never been realised in practice, when the bath was prescribed to persons whose general health had not been primarily impaired. I have ordered patients to continue the use of the bath daily for weeks, even the the sulphur vapour bath, which may be considered the most powerful, and never heard one person complain of its weakening him, but the contrary. It has improved materially the general health, by keeping the surface clean, and by retaining the functions in good order.

The sulphur vapour bath, in cases such as the present, I have found the most beneficial; the mode of administering it, and its peculiar effects, I shall take a future opportunity of explaining.

Tar, and acetic vapour baths, I have also found useful, but not so much to be relied on as the sulphur.

In the constitutional treatment, the bowels are first to be attended to. Repeated purging is injurious. They are to be kept easy by means of any aperient pill, the strength of which is to be suited to the natural irritability of the primæ viæ, in the particular case that applies.

About two drachms of the artificial Harrowgate salts, recommended a few pages back, is what I employ in general; and at the same time administer Fowler's solution of arsenic, commencing with four drops three or four times a day, and gradually increasing the dose.

The solution of arsenic was first recommended by Dr Fowler, and afterwards employed by Dr Girdlestone of Yarmouth, and subsequently by Dr Willan and others. Its use, however, has been much abused; and hence the remedy has very undeservedly been many times abandoned as pernicious and dangerous.

I have now prescribed it in very nearly four hundred cases, and have never yet seen it do any mischief. It is usually given in the decoction of dulcamara, mezereon, or sarsaparilla, as a proper vehicle for its exhibition.

These medicines themselves, have been extolled as specifics, but they do not merit that name; they are very useful auxiliaries, but will not unaided cure one case in fifty. Dulcamara, Dr Crichton, considers one of the most useful medicines that can be given in leprous complaints. He states, that out of twenty-three cases in which he tried it, only two resisted its action. I have given it in three times as many, but cannot say that the result of my experience corroborates his, and find that, in this respect, my observations are not different from those of other practitioners.

Respecting the dose of arsenical solution, it may be observed, that a few drops taken at short but repeated intervals appear to be more useful, and are certainly much safer than large quantities. I have seldom had occasion to prescribe above ten drops three times a day, generally six is sufficient; and in a short time, ten days or perhaps a fortnight, its effects may be observed, not only in the disease, but on the constitution. Of these it may be proper here to say a few words, as connected with scaly diseases. The first symptom that this mineral produces, is a sensible quickening of the pulse, which if the medicine be long continued may be raised even 20 or 30 beats in the minute beyond its usual quickness; it becomes also sharp and hard. In a short time the patient complains of a prickling soreness or itching, attended with a puffy swelling of the eyelids, of the lower one in particular, or the eye is surrounded with a slightly livid circle. Often these are the *first* symptoms, and even premonitory of the quickening of the pulse. If the medicine be still continued sickness at stomach, uneasy feelings of the bowels, and occasional, stitches in the chest, and griping pains in the belly are complained of; the tongue is white, and the expression of the countenance becomes visibly changed, a peculiar grave expression, as it were, being imparted to it; and there is a sense of anxiety in the præcordia, and frequent sighing. But when these latter symptoms are induced, the arsenic has been persisted in too long: there is no necessity to continue its use beyond a cognizable quickening of the pulse, and uneasy stiffness in the eyelids.

Dr Girdlestone informs us, that in his practice he had found the incautious administration of arsenic produce erysipelas and phlyzacious boils.

The farther effects of arsenic need not be here mentioned. When it produces pain in the chest from the first, its employment is precarious, and the murias hydrargyri may be substituted for it. This is given in the dose of one sixteenth of a grain, three times a day, dissolved in any of the foregoing vehicles, and is to be persevered in until some very slight effects are produced upon the gums. Not that salivation, though practised by the older physicians, is at all requisite, but we judge by this criterion that the system is under the influence of the medicine.

The corrosive sublimate is the only mercurial preparation that seems to have any effect upon the skin, and in the diseases of this part it is always to be preferred.

On the *modus operandi* of arsenic, and the above medicine, nothing certain can be affirmed. Their good effects are in general attributed to their virtues as stimulants; but were this the true theory of their action, other and more powerful stimulants should possess equal effects, yet they do not.

I am inclined to think that their immediate operation in curing cutaneous diseases, is the effect they produce on the secretions of this organ, but how or in what way they change or influence these, I can offer no explanation.

We have now to consider the treatment of leprous affections, *accompanied with constitutional symptoms*, or when *the local disease is inflamed, painful*, and attended by general disorder.

But this state we have shown may arise in two ways. The general symptoms may either be such as have been recently induced by some internal agent, and those from whence the external disease indirectly proceeds, (in which case, the local active symptoms may be denominated *primary*), or the general symptoms, to the relief of which the external disease supervened, may have long since subsided, and what exist, as well as the active state of the eruption, may be the result of extensive and severe disease of texture, or in other words, the urgent symptoms may be denominated *Consecutive*.

Under the first circumstances, *i. e.* when the constitutional symptoms and local irritability are *primary*, the indications of cure are, *first* to remove the general irritation; and, *secondly*, to subdue the local activity. The first of these is to be effected by such remedies as the exigencies of the case demand; if they result from acrid ingesta, emetics and purgatives are proper. Antimonial preparations are most eligible as emetics, because they not only deplete the stomach, but diminish the activity of the general circulation, and indirectly that of the skin, and thus reduce the local irritability.

If there be fever, full pulse, or any general symptom that warrants blood-letting, the greatest benefit often accrues from its employment; but supposing that there exist no general symptoms, still this mode of treatment is very often proper, were it had recourse to with no other view than to subdue the irritability of the skin, or its extreme susceptibility to disease. But it has another good effect; it induces a state of the system that admits of being much sooner affected by the use of arsenic, when the active symptoms have been so far subdued as to allow of the employment of that medicine.

With respest to blood-letting in cutaneous affections, I have imbibed some peculiar notions, which it may be proper here to explain. But I may premise, that these views rest on a foundation still liable to objection, and will not bear too strict a scrutiny; yet they have forced themselves upon me, as the result of observation.

First, then, the skin is so highly vascular, and an additional quantity of blood so easily finds ingress into its extreme vessels, upon the slightest impetus given to the general circulation, from the sympathy that exists between the surface and the internal parts, that, upon comparatively trifling occasions, the skin is thrown into a state of disordered action. There is therefore in this organ what may be denominated *natural susceptibility to disease*, which of course, will not only render it more liable to derangement, but will at the same time render that derangement more troublesome when once established; and from its being in a manner merely an extension of a natural action, the cutaneous structure will not be subjected to any great shock or effort, in accommodating itself to the change of action, and which will hence readily pass into what may almost be styled an *adopted morbid action*. So that in subverting this action, we have not only disease to overcome, but, in addition to this, the obstinacy imparted to it from the want of the efforts of nature to remove an action that is of comparatively little inconvenience to her. We have therefore in using the lancet, to bleed largely, to overcome not only the disease, but the proclivity to it; and we find, accordingly, that unless blood-letting is carried to a very considerable extent in leprous affections, it has no effect in relieving the itching, soreness, or other uneasy sensations that often attend them. It is comparatively rare that blood-letting is necessary to relieve the constitutional symptoms, but merely to subdue the external disease; and the detraction of a quantity of blood, which in other inflammations would effectually remove them, has, for the reasons just enumerated, little or no effect in removing that of the skin. This doctrine may be easily exemplified. Every one has remarked the difficulty of removing chronic inflammation of any standing. Witness that of the urethra in gleet. So many causes contribute to keep up this irritable disposition of the true skin, that when once this becomes affected, it is a matter of surprise that a complete recovery should ever take place. Besides its extreme vascularity, the exquisite sensibility of its nerves cannot fail to augment the irritation of these delicate tubes, whilst the operation of foreign agents, of heat, cold, friction, acrid decomposed perspirable matter, must continually tend to aggravate the orgasm so produced. Above all, the crusts formed by the squamous diseases themselves being no longer organised bodies, nor partaking in the general circulation, must increase every day in hardness, and rigidity, and consequently prove an accumulating source of irritation, superadded to the rest; and if we annex to all these the friction of clothing, often ill adapted to the excitable state of the skin, and the constant motions of the latter in the necessary movements of the trunk and limbs, we shall see, that to grant, what no one denies, high sensibility and vascularity to the skin, is to concede the elements which demonstrate its susceptibility in assuming, and its pertinacious irritability in retaining, disease. How much stress the squamous base suffers in every flexion of the trunk, is abundantly evinced when the disease is accompanied by rhagades or chops. In that case, every movement produces a cutting sharp pain, and often hæmorrhage in the fissure.

I am aware it has been argued, that *chronic* inflammation of the skin is difficult to remove, because the cutaneous circulation is so far *out of the influence of the general vascular action*, that blood drawn in such quantity as to affect the heart and larger vessels even considerably, would have comparatively little effect on the superficial capillaries. But how contrary is this to the results of daily experience? Do we not know that the slightest cause which scarcely perceptibly affects the larger vessels, produces a florid and deep blush on the surface from the turgescence it effects in the capillaries? Thus, mental passions will cause a suffusion of the face and chest, without producing any equivalent action in the larger arteries. While, on the other hand, a moderate bleeding, or a mucous diarrhœa of a day's standing, will blanch the most florid complexion to a remarkable degree of paleness.

But I would observe, that blood-letting is not to be indiscriminately employed, and of course can only be used in a *considerable* extent, when the state of the system rather indicates it than otherwise. If there exist any symptoms that contraindicate its use, I need hardly add, that its employment, particularly as here recommended, would in such case be highly improper. The minute details, and discrimination necessary to fix the cases adapted to this practice, would hardly be admissible in an article of this sort, and are better referred to actual experience.*

In addition to bleeding, if the local irritability be great, nauseating doses of antimony, so as to keep the patient really sick for a day or two, have with me proved of great utility; and the daily use of the tepid or simple vapour bath, the former at the temperature of 76° or 80°, and the latter at 96°, F. for I have found the use of a very hot-bath always injurious in acute cases. When by these means the active symptoms have been subdued, the treatment, by stimulating local applications, such as are calculated to subvert the morbid action, and the internal use of arsenical solution, are proper. When the scaliness has disappeared, and the skin is left in a harsh and rough state, the *solutio muriatis hydrargyri* in *alcohol*, or a pretty strong mixture of *solutio arsenicalis*, and *decoctum dulcamaræ*, † used as a tepid fomentation, seems to possess some power in restoring the natural texture.

We have finally to consider the local disorder, when reacting upon the constitution.

This severe state of squamous disease is comparatively of rare occurrence, at least it is rare in this country. I have only witnessed two instances of the sort, the one a case of *lepra nigricans*, and the other of *psoriasis inveterata*. Under such circumstances the mitigation, of course, of the external symptoms, is to be the first thing to be attended to. Relieving these must, it is evident, mitigate the general symptoms produced by it. The same remedies as before enumerated are useful, but no rules can be laid down for their employment. The arguments for and against their use must be left entirely to the judgment of the practitioner; and the symptoms which may be induced, such as were enumerated in the history of the disease, must be treated upon general principles, attention being at the same time always had to the state of the eruption.

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* On a late visit to Dublin, I was informed by Mr Wallace, an eminent surgeon of that city, that for a considerable period he has been in the habit of employing bloodletting with success, in the treatment not only of squamous diseases, but of cutaneous disorders in general. Dr Graves of the same city likewise stated, that he has frequently had recourse to similar practice, and with decided advantage. In a late Number of the Lancet, a case of Psoriasis, treated successfully in this manner, is also recorded. I merely allude to these facts, to corroborate the substance of the practice inculcated in this communication, and to disclaim my having derived it from any of the sources mentioned, it having been my practice at the Public Dispensary for cutaneous diseases of this city, almost since its commencement, and was made generally known by being particularly insisted on, in my Lectures of last Session.

† Half an ounce of the former to lb. ii. of the latter.

A cachetic state of the system is, more frequently than any other, met with under these circumstances; and its removal demands the tonic plan of treatment. Indeed, if at any period of the disease, the state of the constitution demands invigorating medicines, they must be employed; but I have not often seen cases, wherein *the general health called for bitters, wine, and other tonics, when the local disease indicated an opposite mode of treatment;* yet it is perfectly possible that the habit of body may be very much debilitated, whilst the irritation produced by the reaction of the local disease, upon the parts on which it is situated, is so great as actually to induce general fever. This was finally the case in an example of *lepra nigricans,* which occurred to my observation some little time ago, where every patch broke out into deep and active ulceration, and rendered the woman one of the most miserable objects I ever beheld.

9th August 1825, 21, *Duke Street,*
Edinburgh.

II.

Cases of Malconformation of the Urinary and Genital Organs.
By Richard Martland, M. D., Blackburn, Lancashire.

Jonas Wood, æt. 39, of a full habit of body, applied to me, December 24th, 1824, in consequence of retention of urine. From birth, he had a *stillicidium urinæ,* from an irregular fungoid tumour, situated on the outside of the abdomen, in the region of the pubes. It was nearly of an oval shape, the perpendicular diameter being two, and the transverse three, inches. It appeared to be girt by a ligament, was extremely tender, of a lively red colour, and evidently in a high state of vascular excitement. The tumour was covered with a thick coat of mucus, and much swollen. These circumstances in conjunction, most probably closed the apertures whence the urine usually issued, and thus caused the retention, which continued till the 2d January 1825. During this time, his belly was much distended, he had considerable fever, and, when not excited, he sunk into a comatose state. Fomentations and emollient cataplasms seemed to irritate rather than soothe; nor was any benefit derived from the medicines administered to him. The inflammation at length yielded to an application of zinc ointment spread on lint; and after nine days of suffering, he parted with three quarts of urine in the course of one night. The following day all his bad symptoms disappeared, and he then only complained of great prostration of strength.

On the 21st his urine was again retained, and this, in conjunction with a severe diarrhœa, terminated his existence on the 28th. I did not visit him during this attack, but was informed the symptoms were nearly similar to those of his former one. He had a large *prolapsus ani* at the time of his death.

Sectio Cadaveris.—The tumour above mentioned was, on inspecting the body, found to be girt by a firm ligamentous membrane, and was of irregular thickness, the medium being about one inch and a quarter. Its structure was that of a thick spongy membrane. By external examination, no opening could be discovered; but on passing a probe from the inside of the abdomen, through the ureters, they were found to perforate the lower edge of the tumour, at the distance of one inch and one eighth from each other. The ureters, in their course, were very tortuous, much distended with urine, and in some parts strictured; the medium circumference was about two inches; each ureter, with the pelvis of the kidney, was found capable of containing eight ounces of fluid, without any forcible distension. Both kidneys were much larger than usual, and lobulated. Externally, there appeared no umbilicus; but internally, just above the tumour, was a cicatrix, where the umbilical ligaments terminated, showing that at birth the funis had passed through that part. Immediately below the tumour, and separated by a deep fissure, was the penis, one inch in length, and covered with cutis, but destitute both of prepuce and urethra. Its upper surface was flattened, and nearly triangular, with a slight groove running up its middle. The other sides were very irregular. The *crura penis* were of considerable length before they united, owing to the bones of the pubis being five inches and a half asunder. A strong ligamentous membrane connected the bones together. The distance between the two anterior superior spinous processes of the ileum was eleven inches. The pelvis was consequently flattened and widened, and the thighs considerably separated, which gave him, when living, an unsteady straddling gait. The testicles lay in each groin like ruptures, and were of the usual size, but not pendulous. The *vasa deferentia* and the *vesiculæ seminales* were natural. No prostate gland was discovered.

This examination took place in the presence of Messrs Bailey, Cort, and Skaife. We were prevented by the relatives from proceeding to a more minute investigation.

I have recently had an opportunity of seeing another case of mal-conformation of the urinary and genital organs, though considerably different from the preceding. It occurs in a strong man of this town, James Barlow, æt. 29, who wears a female dress, and it is said to have been exactly, or nearly, in the same state from birth. This I was at first disposed to doubt, thinking that what, on the tumour, in other cases, exhibits the appearance of mucous membrane, might have been in that state at birth, and have gradually assumed the cuticular appearance afterwards. His mother positively asserts there has been no change in the skin (as she properly terms it) which covers the tumour. But I should first give you a plain statement of the case.

The tumour in the pubic region is irregularly circular, about two inches in diameter. It forms, or appears to form, a sort of hernia, and when much distended it rises two or three inches above the surface of the abdomen. There is very little tenderness, and the circular band of fibres described in the other case, may be distinctly traced by the finger. The external surface resembles the adjacent cuticle, except about the termination of the ureters on its under surface, where it assumes the fungoid appearance, more particularly on the left side, and these parts are covered with mucus. There are a few very small patches of mucous membrane round the edges of the tumour, which have not assumed the cuticular appearance. The ureters are about $1\frac{3}{10}$ inches asunder, and are very perceptible; the right one will admit a common sized probe, but the left is considerably larger, owing to the passage of three calculi, two of which were of the size of small horse-beans; they were hooked out by the head of a common stocking needle. There is at least another about two inches from the orifice, which is occasionally forced lower down; it is to be removed when a sufficiently small pair of forceps is procured. The urine, which is usually rendered by drops, and is constantly dribbling away, may, by pressure on the tumour, be forced into a small stream.

Immediately below this tumour is situated the penis, a full inch in length when flaccid, and two when erect, as it often is when he has any venereal desires. The glans is well formed; there is also a frænum, and the lower part of the prepuce, but the upper part, as well as the urethra, is wanting. The mucus from about the ureters, lubricates the upper part of the penis. The testicles are pendulous and natural, and the symphysis pubis perfect.

Blackburn, 20*th April* 1825.

III.

Observations on some of the Accidents of Infanticide. By P. J. Martin, Surgeon, Pulborough.

The following abstract, as copied from the public papers, will be the best introduction to some observations upon the case itself, as well as upon some of the accidents of Infanticide in general.

Sussex Assizes—*Lewes, August 2d*, 1825.
(Before the Lord Chief Justice Best.)

" Sarah Little, a married woman, was indicted for the wilful murder of her male infant child, at Pulborough, on the 14th May last. Mr Andrews conducted the prosecution, and Mr Adolphus the defence. It appeared that the prisoner was servant in the family of a printer at Worthing. Being in the family-way, she received notice to quit her place, and her mistress kindly lent her some baby-linen for her confinement, which was intended to take place at the house of a relation. On the day mentioned in the indictment, the prisoner became a mother, in a field near Billingshurt, after having left a house in Pulborough, where she had taken lodgings. On returning to her lodgings, her landlady, observing the alteration in her appearance, and that she was extremely ill, asked her what had become of her child? At first she denied that she had had any child; but upon being pressed more homely, she said, that upon getting over a stile, the pains of parturition came on, and that the infant having fallen to the ground, it was killed upon the spot, and that she had buried it in a ditch. In consequence of her description of the place, the child was afterwards found covered up in a ditch with mud. The child's head was fractured, and some of the mud appeared to have found its way into the windpipe, as far as the cells of the lungs. The surgeons examined, gave it as their opinion, that the child's death might have been occasioned in the manner stated by the mother, and that, supposing the child to be apparently dead from the fall, it might have sufficient life to imbibe, by convulsive respiration, the liquid mud found in the windpipe and lungs.

After an able summing up of the case by the Learned Judge, the Jury found the prisoner not guilty."

The medical evidence here alluded to, stated, that there were several fractures of the bones of the head, great extravasation of blood within and without the membranes; and that, from a laceration of one of the posterior lobes of the cerebrum, brain was found mixed with the extravasated blood: but that, with all this internal mischief, there was no abrasion of the skin, and only two slight marks or bruises on the scalp, one on each side, so slight as would have escaped notice under ordinary circumstances.

That the trachea, bronchia, and air cells of the lungs, were found filled with liquid dirt, a little of which also had entered the œsophagus, but not the stomach.

These were the material facts; and the inferences drawn from them were, *first*, that no ordinary accident short of wilful violence, could account for so much injury done to the head; and, *secondly*, that the child was still living and breathing when placed in the mire.

It is unnecessary to enter into a detail of all the circumstances by which a verdict of acquittal was procured against such strong presumption. Although it was proved in evidence that the child, instead of being born at or upon the stile, was brought forth in another part of the open field, it was so little insisted upon by the prosecuting counsel, and so slightly glanced at by the judge, as in all probability to have escaped the notice of the jury, as it did of the newspaper reporter. And the gist of the medical evidence was avoided by two assumptions, the gratuitous nature of which may be pardoned for its humanity. "That supposing it did not fall from the stile, the mother, in her agony, might accidentally and unconsciously trample upon her offspring, and that then, believing it to be dead, she concealed it in the ditch."

The question, of how much injury is likely to be done to the head of a new born infant, by an accidental fall from the arms, or the body of the mother, has been pretty fully discussed in the books which treat of this subject. Much will depend upon the circumstances peculiar to every case, the strength and bulk of the fœtus, the greater or less probability of a sudden protrusion, the situation of the injury, and so forth. Two instances can scarcely happen alike; and in the absence of all rule, the practitioner is left to form his conclusions from the sum of his observations.

The other point in this case is of more consequence, not only because it may admit of some dispute, but also because it is likely frequently to recur. The smothering of an infant in a privy, in drains, or in any other dirty water, is of too easy execution not to be a frequent occurrence. To settle this point, and to confirm the opinion first formed, that the mud was inhaled, and did not infiltrate after death, the following experiments were made; and although they are not new, except in the material employed, they seem to be more decisive than any the writer has met with upon record. The ready detection of the liquid mud in the substance of the lungs, gives it a superiority over coloured fluids which escape readily from the trachea and bronchia, and are not so easily distinguished from the livid or party-coloured substance of the lungs.

The opinion, once so obstinately maintained, that in all cases of drowning, water did not enter the lungs, has long been relaxed; and it is now generally allowed that some water, mixed with frothy mucus, is found in most cases. For the first few minutes after immersion, it is probable that the contraction of the glottis may be so perfect as to exclude any fluid less rare than atmospheric air; but it is reasonable to conclude, that when the sensibility of the trachea declines, along with the declining powers of all the other organs, it yields to the last respiratory efforts of the sufferer, and admits whatever is presented to it.

A rabbit was drowned in a trough filled with dirty water, made to imitate a muddy ditch. About twenty minutes after death, the lungs and trachea were removed; the bronchia were found filled with dirt and froth, and the air-cells of the lungs were injected with dirt.

Another rabbit was killed and placed in the same dirty water, with a gag to keep the mouth open, and allowed to remain twelve hours. The trachea and bronchia contained a little mud, but the smaller air-tubes none, and the air-cells none.

These experiments were repeated with a very little variation. A cat was dashed against the wall and placed in the trough in a state of insensibility, and died with so little struggle, that it seemed to make no effort to breathe. The lungs were nearly in the same state as the rabbit's in the last experiment, a little dirt had found its way into the bronchia, and some of their larger branches.

Another rabbit was drowned without being allowed to come to the surface at all, after immersion. The lungs were filled with mud, as in the first experiment.

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It is to be feared that the time is not far distant when the public mind will be so much roused to the increasing frequency of the crime of child-murder, that a more strict and exemplary administration of the law, than has obtained of late years, will be called for; and the pardonable leniency of judges and juries will be obliged to yield to the higher considerations of the justice that is due to outraged humanity, and the necessity of giving more effectual protection to innocent blood.

It would be foreign to the purpose here, to enter into a discussion of the policy of the law which visits all descriptions of murder with equal punishment; and why all the parties concerned in a prosecution for child-murder, are invariably found to be in league to defeat its sanguinary purpose. But that this is the fact, the law-reports of almost every newspaper will testify. So little sympathy is felt for the subject of offence, and so much commiseration for the unfortunate offender, and so few social ties are broken, that it is only by a sort of abstract reasoning, and the remembrance that the shedding of blood is an inexpiable offence, that men can bring themselves to feel as they would in a case of adult murder, a crime certainly of greater moral atrocity in its immediate consequences, as it regards the well-being of society.

It is therefore certain, that till an overpowering conviction of the necessity of making examples (as they are now doing in regard to horse-stealing), exists in the minds of those who have the administration of the laws, all the refinements of modern legal medicine, or of anatomical and physiological research, will be useless, whilst prosecutors take no pains to convict, and judges and juries are determined not to believe that a child has been murdered, unless they find it with its throat cut, or its brains dashed out upon the pavement.

IV.

Case of Neuralgic Amaurosis, successfully treated by the Carbonate of Iron, with Remarks. By WILLIAM BELCHER, M.D., Licentiate of the Royal College of Surgeons in Ireland.

"JOSEPH MOXLY, ætat. 32, of the phlegmatic temperament, applied to me, July 15th 1825, complaining of total loss of vision of the left eye, his right possessing that function unimpaired; at the same time he was suffering exceedingly from acute pain of the temple, eyeball, ear, cheek, side of the head, and upper part of the neck of the same side; this pain shooting in *radii* along the *pes anserina*, and affecting the temporal, facial, and cervical branches of the *portio dura* of the auditory nerve, the integuments of the face and scalp at that side tender, and the paroxysm aggravated and brought on by the slightest pressure, attempt at mastication, noise, or even by a current of air. He has had a remission every morning and noon, and a severe exacerbation towards evening. On further investigation, I ascertained the existence of well-marked amaurosis, the dilated pupil immoveable, and inobedient to the stimulus of light, the bottom of the eye of a jet black colour, and a glassy appearance; the superior palpebra was paralyzed and fallen down over the upper half of the eyeball; he had been a fortnight deprived of vision, and twelve days before that had suffered from the neuralgic affections before alluded to. Accompanying these phenomena, he had frontal cephalalgia, *gastrodynia matutina*, bitter taste in the mouth, furred tongue, hepatic fulness, epigastric tenderness, digestive derangement, with constipation, his pulse unaffected and slow, aspect languid and pale, much mental despondency at the idea of being blind so early in life. A month has elapsed, to this date, since the commencement of his indisposition, which he refers to the middle of June, the neuralgia having continued unabated.

The obvious primary indication was to correct the manifold symptoms depending on the derangement of the *primæ viæ*, for which purpose the blue pill, with *ext. colocynth.* 5 grains of each in two pills, were directed at bedtime, to be followed by a saline in the morning; and as it appeared probable that a stimulating vapour directed to the eye would materially assist in restoring the paralyzed retina and eyelid, the *liquor ammoniæ pur.* was ordered, as recommended by Professor Scarpa, until lachrymation and conjunctival redness followed. These measures were continued for three days to July 18th, till the symptoms of digestive derangement were relieved, though the loss of vision and neuralgia remained unabated. The intestinal canal being freed, and the digestive organs improved, I resolved to give a fair trial to the carbonate of iron, first brought into notice by Mr Hutchinson. He was, on the 19th, ordered to take a half drachm dose three times a day, to continue his nocturnal pills, morning saline, and ammoniacal vapour. No change of symptoms until the 23d (the fourth day from the commencement with the carbonate), when the neuralgic paroxysms became less severe, and the upper eyelid was observed to move, though the amaurotic state of the eye was unaltered. On the next morning (the 24th), on awaking from sleep, to his inexpressible satisfaction, he discerned the daylight, and could move his eyelid freely; the neuralgic paroxysm had nearly disappeared. On the 25th (the sixth day from the commencement with the carbonate, having taken 18 doses), the neuralgia had disappeared, pupil now contracts, eyelid moves freely, vision improving, but still indistinct. Treatment regularly continued to the 29th, with daily improvement in vision and digestion; can now distinguish the features of his friends, and even the letters of the alphabet; ammoniacal vapour discontinued; pills, saline and carbonate, twice a day, continued. To August 6th he has daily improved; vision becoming stronger to August 9th. Now nearly well, having taken 46 half drachm doses. Desired him to continue till the 15th. August 15th, he has this morning called on me, and is completely restored to vision, and free from pain, and says his digestive organs are in better order than they have been for a long time. He has taken, in all, 52 half drachms of the carbonate of iron.

Remarks.—This acute disease, first noticed by André at Versailles in 1756; by Louis in 1766; and in 1776, more accurately by Dr Fothergill, who has so aptly termed it "*nervorum morbus faciei crucians;*" and after him by Delpech, Abernethy, Thouret and Pujol; and more recently by Dr Meglin of Strasburg, and Professor Chaussier of Paris, has of late years found a successful remedy in the carbonate of iron, brought into practice by Mr Hutchinson, after having resisted, in by far the majority of cases, the various therapeutic and operative measures devised both here and on the Continent of Europe. Temporary relief has attended several cases, where division of the supra and suborbital, mental, and *portio dura* nerves, as they proceed from their respective *foramina*, has been practised; and in a case which I witnessed six years ago at the Richmond Surgical Hospital at Dublin, where the peroneal nerve was divided, and a portion of its trunk, to the extent of half an inch, cut out a little below the superior *malleolus* of the fibula, for neuralgia of the foot and external ancle, in the course of the anterior tibial nerve and its branches. The sufferings of the patient were completely relieved after the operation, and continued so for some time; but in two or three months after, she returned to the hospital as bad as ever, the divided extremities of the nerve having united, as was imagined. The narcotic tribe had all a fair trial, with but partial relief. She, I believe, left the hospital in despair, and went to the country, as nothing was afterwards heard of her. Mr Hutchinson had not at that time published his successful cases, or she would probably have been permanently relieved. We cannot deny that success has, in a few instances, followed division of the nerves; but I suspect such cases are rare. Dr Darwin has related a most interesting case, where all methods which had been usually practised at that time were tried in vain. He placed the patient under the care of Messrs Cruickshank and Thomas, who performed a series of divisions of the various nerves of the face and lower jaw. The patient, after submitting to the sufferings of seven operations, was completely cured. Dr Haighton has also succeeded in a case of division of the suborbital nerve. Cases of success are also given by Abernethy and Lawrence; yet, notwithstanding these, and a few others, the experience of the present day is against the use of the knife in this disease (the operation is a very painful one, and the relief is only temporary, two powerful objections indeed); and that great and admirable surgeon, Sir A. Cooper, has thus expressed himself relative to this subject. "With respect to the operation for the disease, it ought to be performed rather at the earnest desire of the patient, than at the recommendation of the surgeon." Sir Astley has only met with two cases of complete success from operation in his extensive experience. That accomplished medical scholar, Dr Mason Good, proposes the trial of acupuncture in preference to the knife; we must wait for time to determine the benefit of this. Cases of successful treatment by mercury, pushed to salivation, have been mentioned; and there is *one* in the Gazette of Health for July, where twelve daily electric shocks, each of twenty minutes duration, effected a cure. The *acetic* ether has of late been tried in France, and the prussic acid by Mr Taylor of Cricklade, in Wiltshire, who has found more rapid benefit from it, than from the carbonate of iron. Mr Henry in London Medical Journal for June, mentions two cases of successful treatment, from the external application of belladonna. But I believe Mr Hutchinson's remedy at present supersedes the use of all that have been hitherto tried. Cases have been related of the disease wearing itself out, after a long series of suffering, by Delpech and others; and that respected and valuable physician, Dr Perceval of Dublin, has mentioned the case of his own father, who had been for several years subject to this complaint, and who had tried every thing without relief; He had an issue inserted in the neck, his bowels kept free, went from town to a country residence, and soon threw off the disease. The late respected Dr Parry supposed, that, in the cases of success from operation, related by Dr Haighton and others, the benefit was obtained by the division of the arterial and venous ramifications of the neurilema of the nerve. Sir A. Cooper differs from Dr Parry's vascular theory, and imagines that the action is under par, rather than inflammatory, as he says "the remedies most successful are those of the tonic class, which are known to aggravate rather than moderate inflammatory action." The pathology of this disease is as yet very obscure; but I think, lately, a good deal of light has been thrown upon it, by a very interesting case which occurred to Mr Tyrrell last year. The gentleman died from constant and long continued sufferings. On *post mortem* research, Mr Tyrrell found two fungoid tumours originating from the dura mater; one situated on the right side of the *sella turcica*, closely connected and pressing on the branches of the fifth pair of nerves, particularly the second and third; the other, half the size of a hen's egg, over the cuneiform process of the occipital bone, and connected with the first tumour, by a process of the same matter extending over the extremity of the petrous process of the right temporal bone; the *pons varolii* and *medulla oblongata* were much displaced, and pressed on by the diseased mass. This interesting case, and that of the late Dr Pemberton, in whose brain an ossific growth was discovered, and who, Sir A. Cooper says, suffered more from this disease than any individual he had ever seen, would lead us to imagine, that facial neuralgia was caused oftener than we would suspect, by diseased masses of the brain and its membranes pressing on the nervous trunks destined for the side of the head and face. It is to be confidently hoped, (as the attention of the profession is now more directed to this distressing and obstinate malady, than at any former period in the annals of medical history), that its pathology will be more minutely investigated, and that we shall be able to control its attacks with as much certainty as other painful and less obstinate diseases.

The case of Moxly before mentioned is interesting in two points of view; first, as adding to the already numerous list of cases successfully treated by the carbonate of iron since Mr Hutchinson's publication in 1820; and, secondly, in exhibiting rather a rare complication in the paralyzed retina and palpebra superior, as I believe there have been few cases accurately observed of this complication with neuralgia; and I am surprised at its not oftener occurring, from the extensive anastamoses of the temporal branches of the affected *portio dura*, with the frontal branches of the ophthalmic of the fifth pair. The *levator palpebræ superioris* muscle being principally supplied by the frontal branches of the ophthalmic, may account for its engagement, as also of the retina and iris, through the medium of the lenticular ganglion. In this case, it is to be observed, the neuralgia existed twelve days before vision became impaired; and before any remedy had been administered, he was blind a fortnight. The neuralgia then had continued unabated in its periodical attacks for twenty-six days before any thing medicinal had been prescribed. On his application, the *primæ viæ* being deranged, Mr Abernethy's practice was resorted to, and certainly restored the digestive organs to their tone, without in the slightest degree affecting either the neuralgia or amaurosis. If this had been a case of amaurosis solely depending on digestive derangement, the plan of Mr Abernethy would have succeeded in removing the disease; but the treatment and result prove, beyond a doubt, that the amaurosis and paralyzed palpebra were dependent on the neuralgia. The patient having taken twelve half-drachm doses, the paroxysms began to yield, and the eyelid was observed to move; and, on the 5th day from commencing with the carbonate, when awaking, he discovered the daylight. Professor Scarpa's vapour no doubt materially assisted in restoring the eyelid and retina. That the action of the carbonate is specific in this disease now, I believe will not now be doubted by any man who has given it a fair trial, of course previously freeing well the intestinal canal, and correcting derangement of the digestive organs, if any exist. The immediate *modus operandi* of iron on the system in the cure of neuralgia is, I believe, as yet problematical, and probably will continue so, though it would be gratifying if it could be satisfactorily explained; yet it is sufficient for practical purposes to know the fact, that its action is specific in relieving, and, what is of greater importance, in radically curing a number of cases of this distressing disease. It is remarkable how frequently it has been mistaken for toothach, rheumatism, &c. I have seen a case where several teeth were extracted, with a hope of relieving the supposed toothach, and several cases where all sorts of liniments, blisters, &c. were applied to relieve the rheumatism, without the smallest benefit. I cannot conclude without observing, that the profession and the suffering subjects of neuralgia are much indebted to Mr Hutchinson for his valuable treatment, now, I am happy to say, confirmed on the solid basis of experience.

Bandon, August 16*th*, 1825.

V.

Miscellaneous Medical Communications. By CALEB CROWTHER, M. D., Senior Physician to the Pauper-Lunatic Asylum, and to the General Dispensary, Wakefield.

Remarks on Nervous Energy.

DURING no period of human history, has greater progress perhaps been made in the arts and sciences, than during the last quarter of a century; and the improvement in the practice of medicine has kept pace with the other sciences. Our materia medica has been simplified and improved; greater care has been bestowed in the selection and preparation of our medicines; the knowledge of chemistry has prevented the exhibition of incompatible substances; medical systems and theories have been abandoned; and sounder and more correct views on pathology have paved the way to a more correct and rational practice. In prescribing, medical men have attended to the whole circumstances of the case appertaining to each individual, and not, as formerly, to the name of the disease only.

The improvements in agriculture, horticulture, and architure, by introducing a more wholesome diet and a better ventilation, have, conjointly with the improvements in the practice of medicine, reduced our national mortality. The actuaries to some of the insurance offices have lately been recalculating the value of lives, because the policies of insurance effected upon former calculations have been rendered unprofitable, owing to the increased longevity of the parties insured. Yet, notwithstanding the general adoption of the inductive philosophy, there is less unity in practice, and a greater disposition to change; there is more fashion in the practice of medicine, than in other sciences; there is now, as in the time of Hippocrates, great difficulty in acquiring correct medical experience. Many of our authors write, not to give the result of their practice, but to gain employment, to puff themselves into business. Many practitioners never acquire any experience, or make any improvement, pursuing an uniform routine practice, following implicitly the dictates of their medical preceptor. The continual interruption attending extensive practice is very unfavourable to literary composition; and physicians in advanced life, unaccustomed to wield the pen, are afraid of losing, by their writing, the reputation which they have gained by their practice.

When two inanimate bodies are mixed together under the same circumstances, the same effect uniformly follows; but where an inanimate substance in the shape of medicine is given to an animate body, the effect produced is influenced by a great variety of causes, by functional disorder, by organic lesion, by age, and the quantum of nervous energy with which the patient is endowed. It is therefore very difficult to say when the same medicine is given under the same circumstances. To enumerate the causes that impede the acquisition of correct inferences from medical experience, may serve as a guide to future travellers in the same path, and in this way add an iota to our stock of knowledge. Where an equilibrium exists between the different systems, where the organs are sound, the functions natural, and the age not advanced, the greatest cause of discrepancy in the result of medical practice is perhaps the difference of nervous power with which different patients are endowed. If this statement is correct, the author who could furnish us with a standard by which we might exactly measure the relative proportion of nervous energy which each individual possesses, would contribute more to the advancement of our art, than all the system-writers who have ever existed. Some time ago the science of Galvanism promised to afford a clue towards the discovery of a good neurometer. The progress, however, hitherto made in that science, has not confirmed the expectations which its early dawnings led us to anticipate. What constitutes the difference between the cart-horse and the blood-horse, as he is commonly called? Not any difference in the blood, but in the nervous energy of the animal. The high mettled blood-horse is able to bear pain, labour and fatigue, that would destroy many cart-horses trained in the same way; in like manner, one man endowed with strong nervous energy will weather disease, and escape with life, notwithstanding the most bungling treatment; whilst another, endowed with less nervous power, will sink like a slug on the first attack of disease, although treated very judiciously. What is called knowing a man's constitution, is in fact little more than knowing whether his viscera be sound, his functions natural, and the quantum of nervous energy he possesses. If a physician, when using some new or peculiar remedy for the cure of a disease, chances to meet with ten or a dozen patients endowed with strong nervous power, he succeeds in curing them all, and ascribes the cure to the remedies used, which ought to be ascribed to superior nervous energy of the constitution. During the last thirty years, the most opposite modes of treatment have been adopted for the cure of typhus fever; excessive stimuli, salivation, profuse purging, bleeding, the application of cold water, cold air, and very low diet, have each had their turn and their advocates, and many successful cases have been published from each mode of treatment. Many patients in this disease, who have been most injudiciously treated, have escaped with life, owing to their superior nervous power. To convey in writing our ideas respecting the comparative quantity of nervous energy possessed by different individuals is very difficult. We want a scale and some fixed point to refer to, as in the thermometer. Perhaps tremulous limbs, with a countenance greatly agitated by trifles, might form the freezing point of our neurometer, and complete apoplexy the zero; the power of bearing great fatigue, both bodily and mental, of induring long watching and privations of every description in an extraordinary degree, without any apparent extraordinary muscular strength, ought to favour the summit of the scale. I have met with many medical men, not very well skilled in their profession, who have acquired, by frequent observation, a very correct notion of the nervous energy of their patients, although no more able to detail to you the data upon which their judgment is formed, than the boy who tends a flock of a thousand sheep is able to tell by what means he distinguishes and makes himself perfectly acquainted with the visage of each individual of his flock. Nervous energy is by no means to be considered synonymous with mental capacity. We often find great quickness of perception and acuteness of reasoning in minds endowed with very little nervous energy. Perhaps this condition of the nervous system is more allied to animal courage, than any other object to which we can compare it. It is often the inmate of very slender, delicate looking mortals, whilst the apparently robust athletic man enjoys but a very small portion of it. This, however, is by no means universally the case; we sometimes find a very stout apparently heavy looking man, loaded with fat, possessing a strong nervous energy. It is always the companion of the man of enterprise. The countenance and tone of voice of a patient, his address and gesture, often at one glance give us a correct notion of his nervous energy. The advantage to be derived from a knowledge of the nervous energy of our patient is, that on the onset of disease, we are enabled to ascertain what remedies, what depletion he can bear, and what is the probable chance of recovery. The man that possesses great nervous energy, when affected with acute inflammatory disease, has frequently to contend with a stronger nervous irritation than ordinary men; but he is, at the same time, endowed with a greater power of bearing irritation, as well as depletion. In like manner, patients endowed with very quick perception and vivid imagination, are affected with much stronger maniacal excitement and mental hallucination, than men of inferior mental powers; but in both instances recovery is much more frequent, than in cases of deficient nervous power, and of slow mental capacity.

According to my observation and experience, congestion and metastasis occur most frequently in patients devoid of nervous power, or possessing but a small portion of it. The connection between the nervous and sanguiferous system is as yet but very imperfectly understood. Many observations on this subject have led me to believe, that the congestion of blood, occurring in different organs in various diseases, is frequently the sequel of diminished nervous power in the organ where congestion takes place. The sanguiferous system, unassisted by the proper influence of the nerves, loses its equipoise, and distends the vessels destitute of the natural nervous influence, while in other parts of the body, where the nervous power is natural, the circulation is natural.

In advanced life, we often see apoplexy and palsy occasioned by mental agitation or deep-rooted sorrow. In such cases, there is no doubt that a morbid condition of the brain exists previous to the occurrence of congestion; because we find from dissection, that death sometimes occurs where the mental affection is very sudden and powerful, without any appearance of congestion. We cannot account for this change in the brain by any alteration in the blood itself, or by any morbid or unnatural action of the heart or blood-vessels, or by any perceptible disorder of structure;—the disorder of structure and rupture of blood-vessels, occurring in such cases, is the consequence, and not the cause of congestion. The accumulation of blood in the head, occurring in advanced cases of typhus and mania, is without doubt occasioned by exhausted nervous power, as well as that occurring in apoplexy * caused by deep mental emotion. It is from this cause, in all probability, that maniacs so ill bear the loss of blood. Many persons are said to perish by accidents of different kinds, from want of presence of mind on the approach of danger. Is not this want of presence of mind occasioned by deficient nervous energy?

The necessity of attending to the condition of the nervous energy of a patient, will be best illustrated by the statement of a few cases:—*Delirium tremens* is a disease respecting the treatment of which physicians have widely differed in opinion. Some time ago two cases occurred to me nearly at the same time, which were cured by very opposite treatment; and I am of opinion, that, had I reversed the treatment, I should have lost them both. One of the cases was an attorney-at-law, about 40 years of age, whose nervous energy had been completely shattered by the long continued use of ardent spirits. He had, when I first saw him, a quick, feeble, fluttering pulse, difficult to be counted, with *subsultus tendinum.* He took 14 grains of opium in 24 hours, with wine, cordials, ether, &c.; in a few days he became sane, and was soon restored to the state of health he enjoyed previous to the attack. The constitution was too much worn out, for the patient to be able, had he been willing, to have relinquished the use of ardent spirits. The other was an innkeeper, about 38 years of age, who had been addicted to the use of ardent spirits for about a year: he possessed considerable nervous energy, and sufficient arterial tonicity, to allow the use of the lancet and other means of depletion, by bleeding, blistering, cold applications to the head, low diet, and small doses of opium, this patient was in a few days restored to health. Some years ago, I was consulted by a gentleman, between 60 and 70 years of age, who laboured under some degree of subacute hepatitis; his pulse was about 84, and feeble; he had slight dyspnœa and pain in his right side, little thirst, his hands trembled, his nerves were completely shaken. By the use of blisters, leeches, and saline purgatives, I procured considerable remission of the symptoms. About the 6th day of the disease, he was visited by a medical man, a near relation, who, without ceremony, took away twenty ounces of blood from the arm. The vital powers began to fail immediately, and he died in twelve hours after the operation. Had the same mild mode of treatment been continued, I have no doubt that this patient would have recovered. The biliary obstruction was inconsiderable. The cases which I have related are instances of the morbid condition of the nervous energy. The following is an instance of intermittent fever cured by the exertions of nervous energy alone:—I was sent for to see a female about a fortnight after her accouchment, attacked with the usual symptoms of pyrexia, which recurred every other day, with regular intermissions for several times. Doubting lest the disease might be occasioned by inflammatory action in the uterine system, I administered purgatives and saline medicines. I could not find that my patient had been exposed to the influence of marsh miasma. About the fifth period that the symptoms of fever were expected to come on, my patient, naturally endowed with strong nervous energy, worked up her mind into a high state of excitement, stating, that the recurrence of the cold fit was nothing but the effect of habit, and that she was determined it should not return. She succeeded in the attempt, and cured the fever by the exertion of nervous energy.

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* Where lightning does not produce immediate death, apoplexy is induced. *Vide* Edin. Med. Chir. Trans. Vol. I. p. 360. Is not apoplexy, in such cases, produced by the sudden loss of nervous power occasioned by the lightning?

Supposing the medical treatment to be the same, the difference in the numbers per cent. who recover in acute diseases will be immense, where one half of the patients are endowed with high nervous energy, and the other half have either a shattered nervous system, or have a timid disposition, with very little nervous power. No correct result of the effects of medicine on the human constitution can I conceive be drawn, without taking into account the relative nervous power of each individual patient. Should I, by these cursory remarks, succeed in drawing the attention of physicians to this subject, I conceive that, in the course of a few years, very rapid advances may be made in the art of acquiring experience in medicine.

A peculiar Case of Ulceration of the Bladder, cured by Injection.

In the year 1814, I was sent for to visit a young gentleman, about 20 years of age, labouring under ulceration of the bladder. He had been ill for some weeks when I first saw him, under the care of Dr Billam, of this place. He complained of pain in the region of the bladder, which was augmented when he voided his urine. He had a quick pulse, with irritative fever, and was considerably emaciated. I examined his urine daily, and the quantity of fetid, purulent matter deposited in it amounted, in the course of 24 hours, to between two and three ounces, as near as I could guess. One day, after I had attended him about ten days, on decanting the clear urine from the sediment, in order to ascertain its nature and quantity, I discovered a number of transparent crystals, mixed with the purulent discharge; they were from half to three-eighths of an inch in length, narrow, of an irregular shape, with an edge at one end, and one side as fine as the sharpest lancet. I poured some boiling water from a kettle upon the sediment, and found that the crystals readily dissolved. This experiment led me to the remedy which saved this patient's life. Laxatives, diluents, demulcents, anodynes, with uva ursi, had been given for several weeks without benefit, nay, he lost ground very rapidly. At this juncture, I ordered a pint of tepid water to be injected into the bladder twice a day. The good effect produced was instantaneous and striking; the pain in the bladder, and in voiding the urine, ceased, and the quantity of purulent matter diminished every day, and ceased in one week after commencing the use of the injection. In three weeks from this time he was in good health; he took Soda pills for several weeks after this period; and the complaint, I believe, has never returned.

The ulcers, no doubt, proceeded from wounds made in the coats of the bladder by the sharp spiculæ of the salt or gravel deposited from the urine. I regret that I, in the first instance, neglected to save some of the crystals for analysis. After the use of the injection, I had no opportunity. I have deferred publishing this case, in expectation of meeting with a similar one, in order to procure some crystals for analysis; but nothing bearing any resemblance to it has occurred to me. Mr Jesse Foot was, I believe, the first person who recommended injections into the bladder in similar cases; and perhaps the cases which he cured by the use of injections, might be induced by similar causes as in my patient. In the beginning of this disease, hæmaturia occurred constantly on voiding urine. After I was consulted, its occurrence was less frequent.

Case of Hydatids in the Peritoneum.

In December 1822, Miss P——, aged 30, was seized with sickness, headach, and pain in the lower part of the abdomen on the left side. By the use of fomentations, purgatives, and saline medicines, these complaints left her in a few days, and her surgeon saw her no more until the 11th of March 1823, when her whole appearance was much changed; she had entirely lost her former plump and healthy aspect, and had become considerably emaciated, with dry and shrivelled skin, and a very quick pulse. She made no particular complaint, except of a sense of weight in the abdomen, which was much distended. Fluctuation therein was very perceptible; there was no swelling in any other part of the body. She took diuretics and purgatives without any beneficial effect, and her mouth became sore from the use of Pil. hydrarg. and Ung. hydrarg. I first saw her on the 12th of April, when her appetite was much impaired, her pulse very quick, her urine pale and scanty, the distension of the abdomen was equable, and the fluctuation so distinct as to induce me to consider this a case of common ascites. She took the usual medicines prescribed in ascites, without any increase of the quantity of urine, or other beneficial effects, until the latter end of May, when the late Dr Mossman was consulted, who again prescribed without effect the same medicines that had formerly been tried, not doubting of its being an ordinary case of ascites. Sickness and vomiting, with great debility, followed the exhibition of squill and digitalis; the body continued to increase in size until the 29th of August, when a trocar was introduced, as we supposed, into the cavity of the abdomen, through the left side, and about two quarts of a very thick fluid, nearly the consistency and colour of thick water-gruel, were discharged. A few days afterwards, a larger trocar was introduced into the linea alba, a little below the navel, and about one gallon of the same sort of fluid was evacuated. After this operation, very great hardness and inequality, apparently arising from tumours in the cavity of the abdomen, were distinctly perceptible on the right side, but the bulk of the body was very little diminished. The fluid was so thick as to pass with difficulty through a very large trocar; it coagulated by the application of heat. From this period until the time of her death, the 3d of October, this patient took no medicine, except laxatives and anodynes. Her sufferings, during the last fortnight of her existence, were very severe. She died much emaciated. So little relief was obtained by paracentesis, and her strength declined so rapidly, that a third operation was not recommended.

Dissection.—Twelve hours after death the body was opened. The veins of the abdomen were large, and the skin much distended, and a little discoloured. The trocar with much difficulty was plunged into several parts of the abdomen, and about half a gallon of very thick opaque fluid evacuated. The integuments and abdominal muscles were then thrown back, and the peritoneum laid bare. To describe the appearances, on first dissevering its laminæ, is no easy matter; its parietes were, in different parts, from one to two inches and a half thick, and white throughout, consisting of firmly condensed cellular membrane; an heterogeneous mass presented itself, containing cysts of various forms and sizes, some round, some oval, some transparent, some opaque, some capable of containing two quarts, others not half an ounce. The transparent cysts contained a thick, transparent, syrupy, gelatinous fluid; the white cysts contained a white fluid, of the consistence of thick cream; the coats of some of the larger cysts were very thick and opaque; there likewise existed a quantity of empty cysts, and flakes of coagulated lymph. The whole had the appearance of an immense quantity of purulent matter, floating amidst the hydatid cysts between the laminæ of the peritoneum, and amounted to betwixt six and seven gallons. On removing the peritoneum, no adhesion was found between it and any of the abdominal viscera, which were all sound; the only deviation from a healthy condition being, that the small intestines were darker coloured than usual, and the omentum very thin and shrivelled. No fluid was found in the cavity of the abdomen.

Observations.—The sufferings of this patient, during the latter part of the disease, were very severe; peculiar circumstances, in the early part of it, induced her to conceal her ailments. The urine, though scanty, was always clear and pale, the pulse uniformly quick, the skin dry. These symptoms, with dull pain in the body, may, in the early part of the complaint, serve to distinguish the disease from ascites. The first time I saw this patient, there existed no tenderness in the abdomen; the swelling was uniform, and the fluctuation very distinct. No mode of treatment, at that period, could have effected a cure. In the first stage of the disease, leeches, blisters, purgatives, carbonate of soda, and tincture of iodine, might perhaps have saved the patient.—Can this disease be produced by the exhalents pouring out a fluid more tenacious than usual, in a globular form, somewhat like soap bubbles, which increase in size, and receive nutriment from the surrounding parts after their detachment?

Case of Tetanus, induced by a Spectral Illusion. With some Observations or Apparitions.

In December 182 , I was sent for to visit Mary Irvin, aged about 24, labouring under a severe attack of hysteria, said to have been occasioned by fright. She complained of headach. Finding the pulse full and strong, the habit plethoric, and the bowels costive, I ordered her to be bled and purged, and the disease disappeared the next day.

In December 1822, I was sent for to visit the same patient, labouring under idiopathic tetanus, induced by terror. Her pulse was full and strong; the vessels of the tunica albuginea were loaded with blood; she was delirious; her jaws were locked, and she was affected at times with emprosthotonos. She was bled generally and locally, was subjected to tepid effusion, was purged freely, and in 24 hours the tetanic symptoms left her; she got well in a few days. Her medical attendants, Messrs Milne and Bennett, informed me, that in the interval between my first and second visit, the hysteria had frequently returned. The disease was first induced by a spectral illusion. She imagined that she had seen the departed spirit of the deceased wife of her present master; and several succeeding attacks were occasioned by the reappearance of the apparition. Her master was a widower, having lost his wife a short time before Mary Irvin became his servant. He had had a child by his late wife previous to their marriage; and it was reported to Mary Irvin, that the shame and disgrace which she endured from her husband's breach of promise of marriage, caused her death; and that after such a death, it was impossible for her to rest in her grave, and she must haunt her husband. Mary Irvin living much alone in the house, was constantly much agitated, and her brain was highly excited from fear of seeing this apparition. She was an ignorant credulous young woman, of very strong muscle, and strong masculine form; she was born in the north of England, where she had been accustomed to hard fare, and to labour in agriculture like a man; since she entered into the service of the widower, her work had been trifling compared to the labour to which she had formerly been accustomed, which, with improved diet, accounts for her having become plethoric. After her recovery I recommended her to return to the North, where, by again encountering hard labour and hard fare, the plethoric state of the system would be less likely to return, and her associations would be likely to be weakened by being removed to a distance from the scenes which had brought on the disease. She has not had any violent attack since she went into the North, but has not yet been able to get rid of her fears. After she had seen the apparition a second time, which she told me exactly resembled in person and dress her master's late wife, she consulted with her neighbours learned in apparition lore, who told her that she must speak to the ghost when it appeared again, for that ghosts could not speak until spoken to. On the next appearance of the spectral illusion, she did muster up courage to address it, and received a message from it, which she was enjoined to deliver to her master, and to no one else. She now entertains as firm a belief in the existence of this conversation, between herself and the apparition, as in her own existence. No attempt has been made, to my knowledge, to prove the impossibility of a departed spirit holding colloquial intercourse with a human being on philosophical principles; yet, I think, that it may easily be done, if it be granted that the soul does not possess the properties of matter.

All psychologists agree, that the soul on leaving the body, loses all the properties of matter, and hence becomes denominated the immaterial principle. The common experiment of a bell, *in vacuo,* being incapable of emitting sound, proves this, that to produce sound some impulse must be given to the atmospheric air, from which the sound issues; but, by the assumption, the departed immaterial spirit could give no impulse to the air, and therefore produce no sound; and so far the effect produced by the immaterial principle upon the air would resemble the bell *in vacuo.* To utter language is a much more complex operation than to emit sound. If, therefore, a departed spirit cannot effect atmospheric vibration, it is impossible for it to perform the more difficult operation of talking without lungs, without windpipe, without tongue, and without nerves. Extension, figure, colour, motion, visibility, individuality, animation, and all the properties of matter and life, except tangibility, are usually given to apparitions; yet, from no source of human knowledge, can we ascertain that the immaterial principle possesses any of these qualities.

The belief in the existence of apparitions among the middle and lower ranks of life, is almost as general in this country as the belief in Christianity; nay, many teachers of our holy religion have inculcated such a belief as a necessary part of biblical faith. The impression on many strong minds made by ghost stories in early childhood, has been such, that no future knowledge could remove the associations thus formed, and the impressions of terror thereby inspired. Alderson, Ferriar, and Hibbert, have thrown much light on the philosophy of spectral illusions; but these works are chiefly confined to medical and philosophical readers. The writer who would take the trouble to collect and publish, in some of our more popular magazines, the principal facts and arguments in plain and intelligible language, would confer a benefit on society. The persons who see apparitions are generally beings whose imagination and nervous system have been highly excited by the fear and expectation of seeing ghosts, whose nights have been restless.

The sequel of this preternatural excitement of the brain and disturbed rest is congestion of blood in the head. In this state of the frame spectral illusions are most apt to occur; in some instances, however, they occur where the nervous system has been highly excited, and watchfulness and congestion of blood in the brain, produced without previous association or fear respecting apparitions, as in the celebrated case of the Berlin bookseller Nicholai; but that congestion of blood in the brain occurs in all, is evident from this circumstance, that they are all cured by local or general blood-letting. The presence of spectral illusions is in fact occasioned by a temporary derangement, where the patient mistakes the ideas of his imagination for actual perceptions. This also frequently occurs in the delirium of typhus. One striking evidence of the fallacy of spectral illusions is, that no two persons ever saw a bullet-proof ghost together, where two individuals have slept in a room reputed to be haunted. Each, if he has seen any thing, has seen a different kind of spectre. This fact alone proves the existence of something peculiar, unnatural, or morbid, in the brain of individuals who see apparitions. If any visible object really existed, it would be seen by all situated within the sphere of distinct vision.

The inconsistency of conduct, and the trivial errands on which apparitions are reported to be sent to visit this world, afford sufficient proof that they are not of supernatural origin; their appearance is principally confined to the illiterate and vulgar, and to the man of imagination all compact. No new idea is ever communicated by a ghost, whether it assumes the form of man or devil; its costume and figure accord with the preconceived notions of the person who sees it.

Hints towards forming a Prognosis in Cases of Mental Derangement.

The aggregate proportion of recoveries from mental derangement, in different hospitals and different countries, is already well known. Dr Esquirol states in Dict. des Sciences Medicales, art. Folie, as the general result of different lunatic institutions, that the absolute cures amount to about one-third, and that the number varies from a fourth to one-half. In the Wakefield Pauper Lunatic Asylum, where the patients enjoy, perhaps, superior advantages with respect to free country air, well ventilated apartments, employment out of doors and generous diet, to most of the old established hospitals, the following is the report from the opening in November 1818 to December 31st 1824.

Patients Admitted.	*Males.* 331	*Females.* 321	*Total.* 652
Discharged Cured .	122	144	266
By desire of Friends .	11	24	35
Dead . .	72	45	117
Remaining in the House .	126	108	234
	331	321	652

The proportion of Cures to Admissions, therefore, is as 1 to 2.451.

From the report, however, of pauper lunatic asylums, no inference can be drawn with regard to the general probability of recovery from mental derangement, because a majority of the cases are incurable on admission,—from congenital causes, early improper treatment, or from cerebral lesion which has occurred during the long continuance of the disease. The effect produced by the act of Parliament fining the overseers of the poor for not sending the patients within a fortnight from the commencement of the disease, is very often a false statement with respect to the duration of the disease. Very old cases are often reported to us to be very recent ones. As far as I am able to judge, from the experience which I have had, I apprehend that seven out of ten recent cases, including every species of derangement among the pauper lunatics may be, and have been cured. By recent cases, I mean patients who have not been more than three months insane, and who have not incurred cerebral disorganization, or some incurable visceral disease during that period. From the researches of Dr Villerme respecting the mortality in different hospitals in Paris, it appears that the number of deaths vary from one in twenty to one in four, according as the patients have been well or ill fed previous to admission. The mortality amongst the poor in Paris is in some districts double, in others quadruple what it is amongst the rich in the same city. There can be no doubt that the proportion of recoveries amongst the insane rich in this country, who have all appliances and means to boot, will be much greater than amongst the poor, other circumstances being equal.

Dr John Willis, in his evidence before the House of Commons in 1789, stated, that nine out of ten of his patients recovered, which Dr Francis Willis explains in the following manner. Instead of asserting that nine out of ten cases of insanity recovered indiscriminately, he only asserts "that nine out of ten of that species of the disorder, namely, delirium and derangement *cum febre*, under which the exalted patient more particularly laboured, had recovered, if brought under his care within three months after the attack of the disease. He did not comprehend in those, that determined and settled case of insanity where the mental faculties alone are distractedly engaged, and exclusively of any participation with the body. He did not intend, when he delivered this opinion, to comprise the *mens insana in corpore sano*."—Such a thing as *mens insana in corpore sano*, never, I apprehend, existed; but where there is but slight ostensible corporeal disease, the chance of recovery is much diminished. Not more, perhaps, than one case out of ten of this class of patients recovers, and that after the disease has endured many years.

The circumstances to be taken into account, *generally*, in estimating the probability of recovery from insanity, are the age of the patient, the organic soundness, the nervous energy, the equilibrium of the arterial system, and the quickness or dullness of the intellectual powers.

Individuality.—It will be necessary to consider, whether the disease be hereditary in the patient's family—what is the cause of the disease—what is the usual occupation of the patient—what are his pecuniary and family circumstances—what number of attacks he has endured—whether or not he has been removed from his family connexions, and put under suitable medical treatment and restraint—whether an ostensible corporeal disease accompanies the derangement.

The circumstances which apply generally to all cases of derangement, apply generally to most other diseases. Young people, and those possessing sound viscera, strong nervous energy, and an equal circulation, have a much better chance of recovery from any disease than people in advanced life, and those devoid of the qualities above enumerated; but my experience induces me to believe, that patients endowed with quick intellectual powers, recover in much greater proportion than those who are dull, which observation does not, I think, apply to other diseases; they labour under higher maniacal excitement, and stronger mental hallucinations, than dull men, owing to their greater sensibility; but they ultimately recover in greater proportion than men of slow mental capacity. Pinel says, that persons of the greatest mental excitement, of the warmest passions, the most active imaginations, and the most acute sensibility, are chiefly prone to insanity; and this accords with the sentiments of our Poet, who says,

> " Great wits to madmen nearly are allied."

According to my own observations, however, the poet would have been nearer the truth had he stated, that " great *fools* to madmen nearly are allied." The poignancy of feeling of men of very quick perception, vivid imagination, and highly cultivated understanding, is infinitely more acute than in men of ordinary talents and ordinary acquirements; but their superior good sense, where there is no hereditary tendency to mania, is more than a counterpoise to their feelings; they know the danger of derangement from the unlimited indulgence of irritated feelings; their superior judgment teaches them to check the morbid wanderings of imagination, and to control the indulgence of passion, before the boundary of reason is passed—before the mind is upset. I have heard a patient in this situation make the following avowal. " I felt my mind going; I found that I must either divert my attention from the object which distressed me so heavily, or that I must become immediately deranged; the struggle was a painful one, but I conquered, and trust that I shall derive benefit from experience, and never again allow my mind to be so completely absorbed and exclusively occupied with the troubles of life."

On the other hand, men of weak intellect, when assailed by the ordinary predisposing causes of derangement, soon become confused, overwhelmed, and deranged. I conceive that four-fifths of the inmates of the pauper lunatic asylum at Wakefield are below the level of ordinary mental capacity; and a great majority of my deranged patients in private practice have been of the same description. Although the proportion of recoveries be greater in general in patients endowed with good talents, yet some cases occur where patients of very slow mental capacity recover much more rapidly. Half idiots, just capable of attending to some mechanical employment, are soon roused to madness, and soon recover. Anger, intoxication, or any considerable irritation, in such cases, induces derangement, which soon ceases, on the irritation being removed, the mind in a short time relapsing into its usual state. The want of memory, and weak power of association of ideas, prevent the renewal of mental irritation; and in this way perhaps contribute to the removal of derangement. Among the subjects to be taken into consideration in every individual case, the most prominent is the hereditary tendency to derangement. The effects of severe attacks of mental alienation is a diminution, or total abolition of the intellectual powers. In dementia we find, on dissection, that the organization of the brain is materially injured. In one case, where the patient died from pulmonary disease, a few weeks after he became sane, I found the arachnoid coat opaque, and water in the ventricles. In attacks, therefore, followed by apparent complete recovery of the mental faculties, it is probable that the brain sustains some injury, and that this injury may be communicated to children born subsequent to such attacks. If this inference be correct, a great diminution of the intellectual powers ought to occur in families who have for several successive generations been afflicted with insanity. Accordingly, we often find that, in such families, part of the children only are endowed with good mental powers, the remainder being fatuous, or of weak intellect. In families of this description, we frequently find, where no derangement actually exists, that there is something very peculiar, uncertain and whimsical, in the individuals composing them; they cannot bear contradiction. If thwarted in their views, however unreasonable they may be, they exhibit strong passion and resentment, and render their near relatives very miserable However unsuitable their wants may be to their situation, they cannot be appeased until they are satisfied. My only alternative, said a gentleman to me, who had a wife of this description, is a prison for myself, or a madhouse for my wife. It is obvious that there must exist a considerable difference in different cases, in the prospect of recovery from hereditary derangement. The proportion of recoveries must vary according to the state of the constitution, and the causes exciting the disease; but, upon the whole, the prospect of continuing long sane is much less favourable than when there exists no hereditary taint. I have often noticed, that patients of this description recover from the first or second attack more readily than those who have no hereditary tendency to derangement; but they relapse from much slighter causes, in consequence of their proclivity to the disease. It is excited in all probability by less cerebral lesion. It is in general among the hereditary insane that those intense and formidable cases of mania occur, unaccompanied with apparent corporeal disease, which yield to no remedial treatment, and continue very frequently through a long life. It is remarkable, that there exist very few families that will acknowledge that insanity is hereditary in their own case; it is from other sources that this information must be procured.

It is not by a simple enumeration of causes predisposing to derangement, that a just prognosis can be given. It is from a minute inquiry into all the particulars of each case only, that a correct opinion on this subject can be formed; for instance, erroneous views of religion very often induce insanity, but the prospect of recovery varies much in different cases. Where the mind becomes bewildered amid conflicting doctrines, or delirium and fever are induced by terror respecting the prospect of future punishment, the disease gives way as readily as when excited by other causes producing similar cerebral irritation. On the other hand, when the patient imagines himself to be inspired, and to possess some supernatural prophetic powers, without any apparent corporeal disease, the prospect of recovery is very small. The peculiar tenets of the Society of Friends renders that body of Dissenters peculiarly liable to this variety of monomania. In the Wakefield Pauper Lunatic Asylum, the Methodists have furnished by far the largest contingent of patients who have become deranged from erroneous views on the subject of religion. Again, where insanity is produced by occasional ebriety, ninety-nine cases out of a hundred will recover, and that after frequently repeated attacks, provided that these attacks are not the sequel of daily and habitual drinking, in which case the brain becomes disorganized, and the patient lapses into a state of incurable dementia. Whenever a weak and ignorant mind applies itself blindly and intensely to the study of some obscure and difficult subject, without sufficient previous knowledge to enable the individual to form a correct notion on the subject in question, great cerebral irritation, followed by insanity, is often the consequence. Whether the individual be employed in searching into mechanical philosophy, to discover the source of perpetual motion, in investigating the philosophy of the human mind, or in scrutinizing the mysteries of our religion, the same effect will be produced, but the prospect of recovery will vary very much. Where the vascular system is affected, and much fever produced, the patient will much sooner recover than where the disease comes on very gradually, without apparent corporeal disease, and accompanied with very vivid morbid association of ideas, and strong maniacal excitement, without any increased vascular action. Monomania induced by pride, supposed sorcery, imaginary syphilis, avarice or real pecuniary distress, is very difficult to cure; and the proportion of recoveries, in such cases, is very small. From mania produced by blows on the head, typhus gravior and epilepsy, particularly the two last, very few recover in proportion to the numbers attacked. In these cases, cerebral disorganization is produced by previous disease, and mania is the effect of this disorganization. On this account, when such cases do become sane, they are very prone to relapse. For the same reason, when palsy or apoplexy precede or supervene on mania, the prospect of recovery is very small. Nearly all puerperal maniacs recover, if not too much bled. It very often happens that we cannot trace the first attack of mania to any assignable cause. In such cases, the prognosis must be drawn from the circumstances heretofore or hereafter detailed. I have not in general found such cases very intractable. Where a man becomes deranged in consequence of the immoral conduct of his wife and family, there is but small prospect of his continuing sane; on returning to his family, he speedily relapses. The position of the head in working is very unfavourable for recovery. In shoemakers, particularly where the patients have a tendency to cerebral congestion, or are epileptic, there are but few patients who do not recover from the first attacks, unless they have been very improperly treated. It is from the second or third attack, in general, that the greatest difficulty occurs of removing the derangement.

Notwithstanding all that has been written by the most experienced and scientific writers on mania, the great mass of country practitioners are in the habit of bleeding in this disease to an extent that destroys the nervous energy, and induces dementia, if the patient does not sink from exhaustion. The high manaical excitement, which ought to be obviated by restraint, is considered as phrenitis, and treated by repeated bleedings. To this error, I feel satisfied, that hundreds yearly owe their death.

Another great impediment to the cure of maniacs, is the want of suitable restraint in the early part of the disease. If the patient be not removed from the sources of irritation which induced the disease—from the scenes which gave birth to the delusions under which he labours, it will be almost as difficult to remove the disease, and prevent a relapse, as to heal a sore by the application of blistering ointment. In many cases, where no violence at all is used, as soon as the patient finds himself completely in the power of his keeper, he ceases to struggle, and becomes tranquil. The circumstances which prevent the cure of the insane, are frequently wounded family pride among the rich, fear of expense on the part of the superintendents of the poor, and the delusion that an able medical practitioner is as capable of curing the disease at home, as when removed to a proper asylum, and placed under suitable restraint. In this way a disorder, which was at first only functional, becomes structural, and consequently incurable. The rich delay sending their friends from home, in expectation of escaping the stigma of being a deranged family. Notwithstanding the Pauper Lunatic Asylum here has been open seven years, the number of old incurable cases sent to it continues to be considerable. Mania arising from the application of long continued cold to the extremities, is often very untractable. Melancholia is considered by most writers more difficult to cure than mania. In our Asylum, the cure of melancholia has been often more tedious than that of mania; but, I think, that an equal number of cases of the former disease have been cured in proportion to the number attacked. The salubrity of the situation, the warm baths, the liberal allowance of the Magistrates wish respect to diet, and employment in gardening or agriculture in such cases, sufficiently account for the success in the treatment of this disease. The greater number of maniacs, on recovering, begin to grow fat. Where, however, they grow fat, without any intellectual improvement, and at the same time become drowsy and stupid, incurable dementia is in general the consequence. Mental derangement in every form contributes materially to shorten life, except in cases where strong maniacal excitement is produced, without correspondent corporeal disease. In cases of insanity or dementia which have endured many years, the patient generally dies after a few days illness, without any morbid symptom of disease, except sometimes an accelerated pulse for a day or two before death; although, on dissection, chronic inflammation to a very considerable extent is found to exist in some part of the thoracic or abdominal viscera; at the same time, the tunics of the brain are generally found thickened and opaque, and the ventricles loaded with water. Every physician who has had much practice must have noticed, that in cases of temporary mania, occurring towards the close of phthisis pulmonalis, the consumptive symptoms are suspended so long as the mania endures, and recur as soon as it ceases. The only difference, in the old cases of insanity occurring at our Pauper Lunatic Asylum, is, that where the inflammation of the different organs is chronic, no symptom usually attending such inflammation ever occurs. I have seen a case, where the lungs were very extensively ulcerated, without any mark of disease being manifested until within a few hours of the patient's death. Perhaps the presence of water in the ventricles of the brain, may occasion the suspension of the ordinary symptoms. The great mortality occasioned by diarrhœa amongst the insane has been noticed by many writers. On dissection I have found, that in most of these cases an ulcerated surface, and a large quantity of purulent matter has existed in some of the viscera, sometimes in the lungs, sometimes in the abdomen, and sometimes in the brain, affording abundant evidence of the incurability of the disease. To ascertain what cases of mental derangement should, in a legal point of view, be considered incurable, is a point of great importance, and attended with many difficulties, yet I think it is possible to make some progress in this obscure branch of medical jurisprudence. The duration of the disease affords no satisfactory criterion with respect to the probability of the restoration of reason and sound mind; for so long as quick apprehension and memory remain, the mind may be restored; but when dementia has existed for a year, so far as my experience goes, no hope of recovery can be indulged.

Recapitulation.—From the aggregate number of patients attacked for the first time with mania or melancholia, who are treated properly, and put under suitable restraint, within three months from the date of the first attack, seven out of ten among the poor will recover, and a greater proportion among the rich. In a selection made from the aggregate number of such recent cases of mania as are accompanied with mania or melancholia, where the patient is under 50 years of age, where the nervous energy is strong, the circulation equable, the viscera sound, the intellectual powers tolerable, and none of the causes hereafter recited unfavourable to recovery occur, I have no doubt that fifteen out of sixteen may be, and have been restored to sound mind. General bleeding, and the want of suitable restraint, render a great number of cases incurable. Most puerperal maniacs who are freely bled, die; in other cases, where the patient survives repeated bleeding, dementia is the consequence. Where mania is the sequel of occasional ebriet ninety-eight cases out of a hundred recover, and that after fr quently repeated attacks; but where mania is the result of dail and habitual intoxication, the restoration of sanity is not to b expected; dementia is the result. Where mania is occasione by typhus gravior, or epilepsy, by palsy or apoplexy, or by severe injuries in the head, very few permanently recover; they often become sane for a short time, and then again relapse. Patients having a hereditary tendency, from whatever cause the disease may arise, are prone to relapse. Where mania is occasioned by religious terror, or fear of punishment after death, patients recover as readily as from any other cause of severe mental irritation; but where the mind is intensely and exclusively occupied with the mysteries of religion, and the disease comes on slowly and without fever, the patient imagining himself to be inspired, and possessed of some supernatural prophetic powers, recovery seldom takes place. Indeed, from most kinds of monomania, whether occasioned by religion, pride, supposed sorcery, imaginary syphilis, avarice, or real pecuniary distress, where no fever or bodily disease accompanies the attack, not one in ten recovers. Great cerebral injury is often occasioned by the long continued application of cold to the lower extremities, and mania arising from such a cause is often fatal. Where mania arises without any apparent cause, which often occurs where the disease is hereditary, the proportion of recoveries is often very considerable. From dementia, I have seen but one case of recovery, and that in a very recent one. Such patient sometimes recover to a certain point, remain stationary for a few weeks, and then again relapse. Maniacs recover much mor slowly, and in a very inferior proportion, from the second o third attack, than the first, except where it arises from occa sional ebriety, from fits of anger, or where it occurs in ha idiots. Mania contributes very materially to shorten life in al cases, except such as are unaccompanied with ostensible cor poreal disease. In publishing the result of my own observa tions and experience, respecting the prospect of recovery from mental derangement, I beg leave to observe, that I do not pre tend to give a complete systematic prognosis, but merely to offe a few remarks which I hope may be useful to future labourer in the same field.

Wakefield, 20*th October*, 1825.

VI.

Psychological and Medical Researches respecting the Cures attributed to the Prayers of Prince Hohenlohe (Alexander Von)

By D. C. Pfeufer, Directing Physician of the Universal Hospital at Bamberg. *

After some introductory remarks, the author proceeds with the first section.

1. Basis or principle of the cures effected by religious means by Prince Hohenlohe—mode of application—comparison of them with others effected by sympathy and magnetism, and by Gassner and Francis of Paris.

To be enabled to pass a sound opinion upon any undertaking or event, the investigator must first of all ascertain the form in which it first presents itself, and the features in which it agrees, or in which it differs from other analogous events. A conscientious application, says Dr Pfeufer, of this principle to the reputed miraculous cures in question, would have deprived them of their marvellous character, and established the conviction, that those cures, if true, are to be referred to the agency of the mind on the bodily organs.

This assertion, and the following researches, are founded on the explanation which Prince Hohenlohe transmitted, of the cure of Princess Matilda of Schwartzenberg, to the Magistracy of the City of Würzburg. According to this explanation, there can be no doubt that the whole success of this treatment depends upon unlimited and immoveable faith in God, who can and will help those who pray for what is good for their souls. Purity of soul and serenity of mind are the great requisites for the success of these experiments. On this account such sick are commonly chosen, as have purified themselves from sin, and taken the sacrament; and the first attempts are said to have often failed, because those conditions had not been fulfilled. When the number of those that sought assistance had greatly increased, those conditions were often not in the first instance enforced, but their fulfilment was expressly recommended in all cases of failure of the curative measures.

The process of cure begins with the following questions to the sick: "Do you firmly believe that God will and can help you?"

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* Abridged and freely translated from Horn's Archives for 1822, by J. Clendinning, M. D.

On receiving, of course, an answer in the affirmative, the Prince prays, sometimes silently, sometimes aloud, with hands and countenance raised towards heaven, a prayer in substance, "that God will be pleased to assist this person's distress, and restore him to his best former health; that such help is expected from God, because his Divine Son has promised and said, 'Whatever you shall ask from my Father in my name, that will he grant you;' that the cure of the sick man that believes in God, is expected with the same faith, with which the conversion of the bread and wine into the true flesh and blood of Jesus in the sacrament is believed; that it is expected from Him for the glory of his Son Jesus, and for the honour of the Church, out of which there is no salvation:" Then the prayer continues, "Faith is in our days sunk so low, that ordinary modes of instruction are insufficient, and extraordinary events must happen, in order to restore and confirm the ancient faith." The process then closes with a blessing, "in the name of the Father, Son, and Holy Ghost;" and the sick is immediately asked, "Whether he believes that assistance has been given him?" Another answer in the affirmative being given, he is desired, "in the name of Jesus, to leave his bed," and, as in the cases of gouty patients, "to stand and walk without support." If he staggers timorously, or betrays any symptom of agony or fear, unlimited and firm faith in God is recommended, "and he is told to take courage, and to master himself, for that, through divine grace, he is certainly capable of using his limbs." Should the effects not meet the expectation of the Prince, the sick is encouraged to look forward with hope, because "sick persons are often at the moment unworthy of grace, therefore their cure is not quite completed, but that they must be rendered worthy that God should hear their prayers, by passing through a painful period of purification, and by making farther progress in a good life." In some cases, however, the above prayer is immediately repeated.

Such is the exact form of the curative process. Once only, he added, at the close of the prayer—"and now let your pains depart from you, in the name of God, and of the Son, and of the Holy Ghost." This addition seemed to me owing more to the zeal and vivacity of the Prince's expectations of a cure, than to any thing in the case itself. Each trial was usually made at a time on one person only. I have, however, seen them extended to several at once, owing to the great press of sick. Sometimes, also, I saw only a common blessing distributed, by making the sign of the Cross.

Those attempts always drew together crowds to the places where they were undertaken. Sometimes open places were chosen for the purpose. On those occasions, there was neither silence nor order; and, owing to the impetuous pressure of sick and curious persons, the Prince was often interrupted, and sometimes thrown into agitation. The cures were undertaken without any ostentation or mystery, or particular manipulation on the part of the Prince. There was a warmth, a zeal, and a self-confidence such, that the interest excited by the transaction rose higher every word he spoke, and every one no doubt united, to those of the priest, his own prayers for the recovery of the sufferer.

The Prince seems to think the trial may be made on persons of different sects, even on Jews. I myself witnessed the blessing of the widow of a Protestant minister. Children are said to be particularly proper subjects.

The department of disease which the Prince had originally marked out was rather confined, and comprehended chiefly the diseases called Evangelical (as being named in Scripture), viz. deafness, blindness, lameness, and palsy. In the course of time, the experiment was, however, extended to other complaints, even to the organic. But there was no mention of cure of complete deafness, or of blindness, with destruction of the eyeball, or of crookedness of the spine, or of mutilation of a limb. Under my own eyes, all acute diseases remained equally untouched. The proper subjects, therefore, for the trials were always amongst the host of chronic diseases.

The effect on the feelings of the patients was different, it was said, in different individuals. Many thought that during the prayer, they felt a peculiar moist warmth over their whole bodies; others again heard rushing and ringing noises; others swooned away; most had palpitations; several remained cold, and without any sensation. A paralytic that lay in the hospital and was blessed two days consecutively, thought he would die in the violence of his strugglings, and could not on any terms be persuaded to undergo a third trial. He bore, notwithstanding, the actual cautery with singular firmness, and that too twice in four weeks. In one point most sick persons agree, that during the praying, they think all their sufferings, if not quite removed, at least greatly lessened, on which account in general, none leaves, without the deepest emotion and most heartfelt gratitude, the spot on which he, for the first time for many years, has enjoyed a feeling of health.

As the number of those that sought aid increased hourly, and as the Prince's philanthropy was in a manner pledged, so that not an hour was left him for recreation, and as it became necessary to grant the consolation of this new mode of cure, to sick persons in distant countries,* an hour was therefore appointed by the Prince, in which he would pray for the welfare of the distant sick, and in which at the same time the sick should, along with him, raise their voices to Heaven. "At that hour," said he in his reply to such applicants, "after due confession, and after receiving the sacrament, unite with mine your fervent prayers, in the name of Jesus, and with firm confidence in the infinite mercy, and in the assistance and salvation of our affectionate Redeemer, and at the same time revive within you, with all heartfelt devotion, the divine virtues of immoveable faith, of the most assured hope, together with the firmest trust and warmest love, and with true repentance and the strongest resolutions to lead a better life and exhibit a pious and christian behaviour." From the written accounts of several such patients, it seems, they could scarcely await the appointed hour; the nearer it approached, the more vehement their longing became to perform their devotions, and the greater the ease and the more marked the gradual disappearance of pain they experienced.

Possessed of those results, which are entirely matters of fact, we may be allowed to draw a comparison of those experiments with the cures by sympathy and magnetism; those of Gassner, of Francis of Paris, and of the wonderful child of Kehrberg, have also some points of likeness to them. At bottom, those and all other miraculous cures of ancient and modern times, by conjuring, amulets, holy relics, oracles and decisions of Sybils, of Appollonius of Thyana, of the School of Plotinus, &c., have one and the same character; the efficient power in such cases is the influence of faith, which however, in both healer and healed, must be raised to its utmost height, and overpower every other function and faculty of the soul. Thus also the modern cures partake in the common fate of all those others just alluded to, in as much as the fundamental principle of their efficacy is always misunderstood, and their cause accounted supernatural. In appearance, however, the former differ considerably from the cures by sympathy and magnetism, and from those by Gassner, Francis of Paris, and the above mentioned marvel-working child.

The cures by means of sympathy, as usually effected, presuppose a blind superstition, and profound ignorance of the processes of nature; in addition to which they make no allusion whatever to the goodness of God, or to his Church, nor are purity of soul or serenity of mind amongst the necessary conditions. Magnetism requires, in practice, uninterrupted quietude and silence, and loves magical and mystic forms. Magnetic and sympathetic cures are almost always limited to one individual. The marvellous cures of Francis of Paris consisted, as is well known, in this, that all who were laid on his grave had convulsions, which ceased immediately when the convulsed person was removed, and returned when he was brought back again. Pieces even of his clothes were said to restore lost sight. The convulsed (mostly common uneducated persons) made connected pathetic speeches, and even played the part of soothsayers. Gassner, during his operations, sat in a chair with a crucifix in his hand, and a stole round his neck, round which also was a silver chain from which hung a crucifix, in which a particle of the holy cross on which Jesus died, was enclosed. He and the patient looked and stared at each other; he spoke in a rough commanding tone of voice, with one hand pressed the forehead of the patient, with the other the nape of the neck very strongly, and also often touched the painful part, or shook the whole body powerfully. Then began the probationary exorcism, and the patient was affected with convulsions until the exorcist ordered the devil to leave him at rest. The sick, besides this, got medicines, consisting of a peculiar liquor, an unknown oil, and a balsam. The marvellous child of Kehrberg commenced his career at Priegnitz, at four and a half years of age. His method consisted in stroking, scratching, and striking the suffering part. Whoever had headach was pulled by the hair; deaf persons the child tickled, and blew in their ears. If it had not a mind to speak, it washed its hands in a great tub of water, of which believers drank, and out of which they filled bottles either for drink or for sprinkling on wounds and sores. The father of the child, a smith by trade, soon became a rich man. *

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* Written inquiries and petitions arrived daily from Russia, Spain, France, Italy, and even from Rome, addressed to the Prince. Most of the sick, however, came out of Bohemia, from the Tyrol, and from the quondam Archbishopricks of Treves and Cologne.

The curative method of Prince Hohenlohe, is distinguished from all those by its simplicity, and freedom from all mystery; but particularly by the circumstances of its depending for success altogether on the religious feelings of the sick.

I am therefore of opinion, that the agency that obtains in this case must be considered more purely psychological than in the others above mentioned.

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* Umstandliche und wahrhafte Nachricht, &c.—Circumstantial and true account of the wonderful wonder-working child of Kehrberg, by Dr Spiess of Priegnitz Berlin; 1734.

II. Have sick persons been really cured by Prince Hohenlohe's method? Different views of this subject. Point of view in which they should be regarded.

Respecting the solution of these questions, opinions are divided; and indeed, according to my experience, with good reason. One party rejects, without qualification, the very possibility of success; and another relates, with a sort of extasy, examples of complete cure, which in some measure border on the marvellous. But we must not reject the whole indiscriminately, nor too eagerly run into extremes. We have but to determine in what sense, and form what point of view Prince Hohenlohe's proceedings should be considered and judged of, to see that both parties go too far, and that the truth, as ever happens, lies in the middle.

It is indubitably true, and could be proved before a Court of Justice, that under the inspection of the police, in the general hospital, not a single real cure took place, out of a considerable number of subjects.

The same result was obtained in several private houses. On the other hand it is a most certain fact, that out of this circle several sick persons found themselves relieved immediately after the curative experiments, and some even declared they experienced complete liberation from pains, and restoration of voluntary power which they had lost in some of their limbs.

The question then turns upon the sense we attach to the word "Cure." It must mean not merely alleviation of the present disease, but also complete restoration of health and re-establishment of all the vital functions in their former condition. Medical men distinguish two kinds of cure, the radical and the palliative; the former *(radical)* quite eradicates the disease, and renders its continuance impossible, the latter merely modifies and mitigates its phenomena, checks its advance, and diminishes the danger that might ensue. Subordinate to those is that sort of cure to which the faculty is sometimes forced to recur, which consists in changing the form of the disease, and transferring, from a more noble part to one of less importance, the morbid vital action. The palliative cure is at bottom rather a preparation for curing than a true cure, for the individual is not by its means put in full possession of the enjoyments of health and life. With equal justice do physicians distinguish between the belief, and the actual state, of those that complain of illness. A man may be ill without being sensible of it, and *vice versa*; the former does not perceive the functional changes that impede due exercise of his organs; the latter, though enjoying complete organic health, has that morbid feeling, and seeks a cure of which his frame has no need.

From those considerations therefore, it appears, that Prince Hohenlohe has really, by working on the devotional feelings of individuals, effected cures; and it only remains for us to determine their nature, in order to be enabled to say whether they are of a permanent value, and whether they outweigh the risk of such unpleasant consequences as are known to attend imperfect cures. I must remind the reader, that I have chosen as a basis of my inquiries, only such facts as have been established by legal official investigation, and in fact such as I have for the most part myself witnessed, and that as an unbiassed inquirer. I leave all other facts to stand or fall by their own merits.

Of radical cure by Prince Hohenlohe's method, I do not, from my own observation, know a single case; but I am acquainted with several, in which a striking alleviation of particular symptoms occurred. I relate five cases in point, of which two were treated by myself, and three by another practitioner.

Aulic Counsellor I. has for many years suffered from stiffness of the lower extremities, owing to which the mobility of these parts is greatly diminished, and their motion very painful. He has twice already had paralytic attacks of the whole left side, from which, by constant use of medicine, he completely recovered. The mobility of the lower limbs was, just before he had recourse to the Prince, so limited, that the patient could not walk a step without support;—with support however he could walk so well, that some days before, I represented to him the necessity of again going out and taking the air. This patient expected with great emotion the Prince's arrival from Wurzburg, where he then was. He came at last, and on that very evening at nine o'clock, the patient underwent the process of cure. However, nothing remarkable happened—the patient was consoled by the hopes, that on a future day, after preparation by receiving the Sacrament, the curative process should be repeated. It took place; the patient was ordered to stand, and walk without assistance. After some delay the patient went, first led by the hand by the Prince, out of the door, and immediately, altogether unaided, descended a flight of 14 steps, and after a moment's repose, ascended the same again. The delighted patient met me next day with the assurance, that he now often felt inclined to walk about his room in the course of the day, a resolution to which, he said, nothing in the world could bring him before. However, on the third day he could walk no longer without support; his right leg swelled owing to the efforts in walking, and at present the patient is in such a state, that he is forced again to have recourse to medicine.

M. ANNA Z., 39 years of age, unmarried, has for 14 years been afflicted with disease of the heart, to which palsy was subsequently added, extending over the whole left side, so that for six years past, she, being incapable of helping herself, was of necessity carried, whenever locomotion was necessary. Already, before the prayer was finished, she imagined, as she declared to the Court of Inquiry, that she felt diminution of the pain in the palsied side, and increase of power in her legs. She could now raise herself freely up in the bed, and with the assistance of the Prince, who reached her his hand, she could get out of it. She experienced, owing to gentle extension of the previously contracted sinew of the left leg, a pleasurable sensation, and was beside herself for joy. Her legs, it is true, trembled still, on account of their disuse for many years, as she walked; she went up and down the room however, led by the Prince, and remained an hour out of bed. Next day also those attempts at motion succeeded equally well, and she felt herself as strong as during the first moment;—after some days, however, every symptom returned, and now she is as incapable as formerly of getting out of her bed.

MARGARETHA Z., sister of the last patient, 29 years old, and unmarried, has been for nine years bed-ridden, owing to periodical attacks of epilepsy. The attacks usually come on, after intervals of nine days, several times a day during a period of five successive days. Even in this case the Prince undertook to try his religious treatment. She felt, according to her own accounts, both during and after the prayer, an increased strength, and such an emotion, that after such protracted suffering, she could not tell how she was. She could forthwith leave the bed, and holding the Prince's hand, walk up and down the room and to the street window—she was likewise able to stay a whole hour out of bed. The success of this experiment was most striking in the left arm, which, though paralytic for nine years before, was now so far strengthened, that in the presence of the Police Commission, she could with this arm make any movement in any direction, and could lay hold of hard bodies. However, the Commission remarked, in this arm compared with the other, a greater weakness, and a striking emaciation. But her joy likewise was of short duration. The day on which the experiment was made was a day of remission, and the epileptic attack was not expected before the day following. The convulsions accordingly came on, but very mild; after this however the patient was severely attacked. Next evening the attack returned with increased violence, and in a few days she relapsed into her former miserable condition.

AGATHA L., 34 years old, unmarried, has suffered for several years from complaint of the uterus. This complaint is combined with very acute pains in the belly, from which the patient can gain ease only by drawing the left foot constantly up towards the belly, on which account she has not been out of bed for years. After the prayer she stretched, although not without painful struggling, her left foot out of its curved position, and felt a certain facility of motion in the upper part of the body, and in the arms. After the prayer was said for the third time, the patient felt herself as it were possessed, and scarcely conscious of what she did; collected all her force, and in despite of the intense pain, stood up, and walked, after a minute's support from the Prince, and at his command, unsupported, into the middle of the chamber, then knelt down, thanked God for her salvation, and got up again unassisted. The pains in the belly were, however, greatly augmented, by her attempts to walk. The left foot which she had previously kept drawn up towards the belly, retained no doubt its outstretched position, but she evidently preferred carrying it in its old situation, because stretching it out was constantly attended with abdominal pain. She is now in the same situation as before the trial.

BARBARA L., 29 years of age, servant to a gardener of this place, was received into the general hospital on the 27th of April 1821, labouring under coxalgia. On the 28th of May, after having used various remedies in vain, she was subjected to the actual cautery. The consequence was, that the shortened limb regained its natural length and mobility. Fourteen days before the institution of the experiments about to be noticed, the patient was walking about the ward without support, and spent most of the day out of bed. Two days before, she had even been walking in the garden. Some time ago she had been suffering under irregular menstruation; and, at this moment, she was decidedly chlorotic and greatly emaciated. On this account alone it was that she was detained in the institution, as she was, amongst other remedies, to use chalybeate baths. At the request of the hospital chaplain, a *quondam* Franciscan, Prince Hohenlohe came into the ward where this patient was, and, having performed the usual ceremonies, ordered her to go up stairs to the next story, and return down stairs and into the sick ward again, which she did, declaring that she no longer felt the least pain, although she had previously complained very much to the before named house chaplain.

In respect to this cure, I have to remind the reader, that the patient in question was already cured of her original complaint, a peculiar painful feeling in walking excepted. By desire of the chaplain, the Prince performed the experiment. After the prayer was ended, this patient walked, to the amazement of every one that did not know the history of her complaint, unsupported, out of the ward into the next story, up a flight of several steps, and returned into the ward again. Every one now hurried out of the hospital into the street to announce the wonderful event. Just such an instance, above all, out of an hospital in which the public takes such an entire interest, was wanting to establish the healing efficacy of these experiments. We must not, however, blame the cautious cool observer, if, after well examining the affair, he regard with suspicion the whole matter, and reject every fact not officially ascertained. This maid-servant felt, as she repeatedly, in presence of several witnesses, avowed to me, the acutest pains in the thigh that had been before affected while she ascended the stairs, but had not the courage to express it, or to disobey the orders of the Prince. Unfortunately, she suffered for this timidity—the next day she could not move the thigh, nor could she sleep for several nights. On the inner side of the thigh there supervened an erysipelatous inflammation, with painful swelling, which was not resolved without great care and attention. This patient was not quite restored, nor did she leave the hospital for three weeks after.

It may easily be said that this patient, notwithstanding the subsequent sufferings, still owed her complete recovery of the power of walking to the devotional process; for, it may be urged, if she was cured by the previous treatment there could then be no reason for detaining her in hospital. But granting that she remained under treatment, not as really was the case, on account of menstrual irregularities, but on account of the pain that still remained, or, in other words, from an imperfect cure of her primary disorder, still the physician must take into account the circumstances in which such a patient is to be placed after leaving the hospital. Even when he is convinced of the patient's convalescence, and of his fitness to go out, still the rules of his art require that he should look after him, until he is sure his recovery will be permanent, and that he will be able to encounter without injury the morbific influences connected with his situation.

This much, however, may be concluded from facts established officially, and analyzed without prejudice, that *a momentary, therefore palliative, cure of certain complaints may be derived from Prince Hohenlohe's treatment.*

I have still to consider a fact which excited universal attention, and which, if it were not exposed to objections in many respects, would prove, that even radical cures might, in some cases, be expected from Prince Hohenlohe's method. A hussar of the 2d regiment of this place, had for many years deafness of one ear. A good while since, the hearing of the other ear was, through some accident, affected, on which account he had been repeatedly in the hospital. Every attempt and remedy was without effect, and the military medical board had already declared him unfit for any sort of military duty. This hussar was then, in presence of several persons, civil and military, of undoubted veracity and candour, subjected to the devotional process. Immediately thereon, the deafness, to the surprise of every one, was remarkably diminished, and the hearing gradually restored, so far, that next day, in my presence, he heard every word addressed to him from a considerable distance, and in an ordinary tone of voice; although just before the blessing, he could not be made to understand without bellowing in his ear. At present he can do every duty that devolves upon him, and enjoys perfectly his long lost hearing.

The objection to this cure is, that the complaint was simulated, and that the man would not have been taken for military duty, if the officers of conscription had considered his deafness in one ear as real. Even in this case, it might have been expected that the medical board would appoint so heavy a customer somewhat sooner to some garrison station. Finally, it is asserted, that the cure should rather be placed to the account of the preceding treatment, than to that of the devotional experiment. On the other hand, we may ask, what did this hussar gain in at once laying aside all deception, and using his hearing? His projected release from the army was not effected, and now he must serve out his time.

(To be continued.)

VII.

Farther Remarks on Tetanus. By William Briggs, M. D. Liverpool.

In the last Number of this Journal, there is the history of a case of Tetanus successfully treated by Mr Manifold, surgeon, of this town, with some remarks of mine referring to a case formerly published by myself. I there state, that I had seen the same practice followed in five or six cases, more or less severe; and that I had never known an instance where it had failed, *if properly persevered in;* but that a melancholy case had recently occurred, which, if it did not form a real exception to the rule, (and I was not sure that it did, neither am I now), should serve very materially to qualify and chasten one's views. At any rate, impartiality requires that the evidence should not be either suppressed or garbled.

It was not till the day before his death that *I* first saw this patient; but he had been attended for a week by Mr Nesbitt, surgeon; who, having seen Wright with me, and having also read the case of Gaskell, entered into all my views respecting the treatment.

Here it may be proper to insert what I afterwards collected, when I visited the patient. He was a particularly well formed healthy young man, who had completed the term of his apprenticeship as a wheelwright, on Friday, May 27, and as is but too common on such occasions, was much intoxicated at night. He worked hard all the next day, but was again intoxicated at night; as also on Sunday, (King Charles's day), and, in that state, he went out in the evening to gather oak-branches. The night was cold, and I believe *rainy;* and he lay down on the ground near a brick-kiln, where he slept for several hours; and *from that time he was never free from stiffness and aching in his jaws and limbs.* Still he followed his work regularly, and remained quite sober till the next Friday (June 3d), when he *walked* 18 miles to a horse-race. He was little inclined to eat, when arrived there, but got tipsy again: he returned in some sort of a vehicle, and *worked* all day on Saturday, but got tipsy again at night, and also on the Sunday *evening,* though he had remained sober during the day. On the Monday he was unable to work, but *walked* nearly half a mile. In the evening of that day Mr Nesbitt was applied to, and the following is his account.

" Monday evening, June 6, 1825. T. H. Tetanus. I ordered him powders each containing *hydrarg. submuriat. gr.* 10. *Pulv. scammon. gr.* 8. *Cambog. gr.* 5, and *your* mixture *(Infus. sennæ f℥ xiv. Pulv. jalap.* ℥ *ss. Tinct. Sennæ f℥ ii.)* On the Tuesday evening, in the course of about 24 hours, he had taken five powders and a whole pint of the mixture; he had several scanty stools during the day, with very good effect on the *spasms;* but was much griped, and on that account he took none through the night. On the Wednesday, he took four powders, and ℔ss. of mixture, which operated very well; spasms still less, but *great difficulty in swallowing:* applied a blister to the whole length of the spine, and rubbed in *Pulv. opii* ℥ *i.* In the course of the night he took three powders; a great deal better on the Thursday morning; took three more powders during the day, but none of the mixture; well purged; spasms quite slight; took four powders and lbss. of mixture on Friday, a great deal better. On the Saturday morning he had slept three hours at once, without a spasm, but no medicine taken through the night. I directed a powder to be given every four hours; but when I came in the evening, I found he had only taken one. I immediately gave him another, and strongly urged regularity; he took two more and lbss. of mixture through the night, but without producing any effect. On the Sunday, he took four powders, but could not *swallow* the mixture: had a clyster, *several scanty stools without relief;* spasms more frequent, pulse 120; tried the warm bath: gave him Tinct. Opii. gtt. 40; spasms a little relieved for about an hour, but returning with double force, *you* saw him next morning, and will recollect what followed.

" *P.S.*—I have omitted to say that for the first four or five days the pulse was quiet, also that he took about a pint of Port wine daily."

So far Mr Nesbitt. I do indeed recollect what followed: the patient was carried off in a convulsion the next day; an event for which, I confess, I was not at first prepared, since neither the affection itself, nor the treatment appeared at all to come up to the measure of what I had before seen terminate successfully; though had I but borne sufficiently in mind one of my own convictions, and *that,* as I shall soon show, no recent one, namely that, in tetanus, the *danger to life* is by no means in proportion to the intensity of the outward symptoms, there was enough from the first to have excited my alarms. I more particularly allude to the *quickness* of the pulse; a symptom which, according to the late Dr Parry, is certainly fatal; and perhaps it is so, more even than its *irregularity ;* though I did not clearly recollect this distinction till it was too late. I shall, however, presently take occasion to question how far this excellent physician is borne out by facts in *all* his conclusions respecting tetanus. In the mean time, it may suffice to say, that I was in some measure lulled into security by what he and others have said as to the danger being the greatest in the beginning, and that, if the patients do but live over the first few days, they generally recover. It must be allowed, too, there was not a little, besides, to mislead almost any one, with such recollections as mine.

In many respects, the case bore a considerable resemblance to Gaskell's. Thus, in both, the affection seemed to be *idio-*

pathic; and of this species some physicians, Dr Parry among others, are disposed to think but lightly. (I say *seemed*, and I believe it was really so, though in neither was it absolutely certain; for here, too, there had been a slight wound between the left thumb and index, but it was quite healed, and, on pressing it rather strongly, no more inconvenience was experienced than when the same degree of pressure was applied to the other hand. I thought it prudent, however, not to omit a caustic. These circumstances I had forgotten, when I stated, in my former communication, without reserve, that this was idiopathic tetanus). In both cases, too, the approach was gradual, occupying between a week and a fortnight, from the time of its being first felt, before medical advice was sought, and the patient contriving to follow his work nearly up to the time of his applying; whereas, in the case of Wright, which was decidedly *traumatic*, the symptoms came on in the course of one night, after he had walked to Warrington (18 miles) on the day immediately preceding. In the present case, too, the *weather*, from having been very cold and wet, had, for a few days, been unusually hot. On Monday, June 13th, when I first saw the patient, my own thermometer, in a north-eastern aspect in the street, stood at 80° and upwards, for some hours, and it was not many degrees lower on the next day; but then, on the day on which Gaskell first applied, it stood at 91°, and for some weeks together the heat had been, almost without interruption, greater than is often experienced in this country. Again, the pain was much more at the *lower* part of the abdomen, than at the ensiform cartilage. This is not noted by Mr Nesbitt; but he, as well as myself, distinctly remembers it, insomuch that one of his attendants was constantly occupied in pressing on the hypogastrium, which afforded some relief. In my remarks on Wright's case, I stated, that I had been disposed to regard this as rather a favourable indication; and where it can be looked upon as a *translation* from the one to the other (which in that particular instance it apparently might), perhaps it is really so. It happens, however, that the same has been observed in a majority of all the severe cases that are the best known to me, whether the result was favourable or adverse. Finally, it is a curious coincidence that, nearly about the same period in both, there should have been some *relaxation in the plan of treatment, followed by a dose of laudanum, with a subsequent exacerbation of the symptoms.* From Mr Nesbitt's report, it does not appear that more than one powder, without any of the mixture, was taken for twenty-four hours from the evening of Friday, or again that any thing of the sort was taken after the laudanum on Sunday evening till Monday noon, when *I* first saw the patient. On this point, I would solicit a comparison between Mr Nesbitt's Statement and the Reports for July 15th, 16th, and 17th, in Gaskell's case.

Altogether, then, it will probably be thought natural enough, that, with the leading features of the two former cases so fresh in my recollection, and particularly that Gaskell had been rescued, by prompt and vigorous measures, from the very perilous situation to which he appeared to *have been brought by a suspension of them*, I should have been inclined to hope much from similar practice, in a case so nearly similar, only to all outward appearance much less strongly marked—as in truth I was; and the more so, from Mr Nesbitt's Report of the ground actually gained for the first four days; also that the patient had already survived what, according to Dr Parry and others, is the period of the greatest danger.

Seeing, however, that *comparatively moderate doses* had before been found, in this case, equal to the occasion, I did not see any necessity for pushing them so far as I had done in either of the former. And here, perhaps, I was wanting, for in all such cases there is no time to be lost. But be this as it may, I merely directed the scammony in each powder to be increased from gr. 8 to 10, and the camboge from gr. 5 to 8, and *ol. croton gtts. ii.* to be given with each. How many of these he got taken from the time of my first seeing him till his death (which was about 26 hours) is uncertain. It was *said* he took 7 powders and ol. croton gtts. 6; but Mr Nesbitt is confident that not *one half* was got down.

But even supposing the whole quantity prescribed, from the first, to have been taken *bona fide*, and without waste (of which, however, there is not the same satisfactory evidence as in either of the other cases), the total amount for nearly seven days, up to the time of my seeing the patient, does not exceed, calomel gr. 240, scammony gr. 192, camboge gr. 120, mixture lib. 2½; whereas Wright, a boy of 15, and by no means stout, *reckoning only what we administered ourselves* (and, indeed, whoever considers the difficulty there must necessarily be, in getting a patient with his jaw locked, and subject to convulsive catchings, especially on attempting to swallow, to suck a nauseous powder, mixed with treacle, through his teeth, will be convinced that scarcely any thing less than ocular demonstration is to be relied on), took in the same space of time, *before he got the croton*, calomel, and scammony, ā gr. 300, camboge gr. 126, and upwards of lib. 2 of mixture, part of which had been made stronger than ordinary.

But the chief difference was in the *manner of timing* the remedies. In the present case, the practice was the most active at the *first*, and apparently with unqualified good effect; but afterwards it was slackened, contrary to the urgent injunctions of Mr Nesbitt; whereas in Wright's, there was never any thing like a slackening, but rather it was pushed, almost daily, farther and farther; insomuch that, on the morning of the 23d, before the croton was given, he took at twice, and within three hours, what was more than equal to *five* of the doses prescribed for this patient, namely, calomel and scammony ā gr. 50, camboge 25, besides a considerable dose of the mixture, and all this without any sort of effect, till he got the croton and clyster, and in the course of one week he *walked* to the market. (Here I am requested by Mr Manifold to supply an important omission in the printed Report for the morning of the 23d. He generally saw the patient four times in the day; and that morning he had seen him at eight o'clock, and had given him a powder consisting of calomel and scammony, ā ℈i., camboge gr. 10; and when we met at eleven, he gave him a dose and half, making together the quantity I have mentioned. *)

Such, then, is the evidence in this melancholy case. The question is, Does it furnish any valid argument against the free use of drastics in tetanus? I confess I do not see it. If, on the *contrary*, it were asked, whether, if the practice had been diligently followed up, the result might not have been different, I should have much more difficulty in answering. Indeed, Mr Nesbitt has repeatedly expressed to me his conviction, that in all probability it *would*: but he says, that in proportion as the necessity for active practice became less apparent to the patient and his attendants, the less able was he to get his own directions enforced.

On this head, however, I have now my doubts. Even on my own principles, if it be a considerable point gained to get the scene of conflict removed from the citadel to the outworks, and to follow up the first impression vigorously; it cannot be less necessary to attend, at the same time, to the condition of the *citadel* itself, more especially if, through the mismanagement of those left in charge of it, its powers of defence have been weakened. It may be a means of averting irremediable ruin, if, by succours thrown into the garrison, it be enabled to hold out even for a few hours. Now this patient had, by his way of going on, alternately working hard and getting drunk, *even after the symptoms of the disorder had begun to show themselves*, made large draughts on the *vis vitæ*; and the more so, as it does not appear that his frame had been inured to them. In fact, he had reduced himself to what a Brunonian would have termed a *state of indirect debility*. It is true, Mr Nesbitt states he took about a pint of Port daily. It is also true, that neither Gaskell nor Wright could *bear* much wine; with the *latter* it disagreed so much that it was laid aside; and no doubt this circumstance contributed in part to make me think less of it in the present instance; but then, Wright was a boy, with his vital powers unimpaired by excess; and as to Gaskell, he actually *did* take a bottle of Sherry in twenty-four hours, when he was at the worst, and " it always seemed to refresh him, and particularly " to promote sleep; " yet he was not an intemperate man. Dr Currie's patient, who was cured of *traumatic* tetanus by wine, and who also is stated to have been a steady man, " contrived to suck in a quart of Port during the first two hours, and three bottles ditto in the course of twenty-four hours. " I had myself, also, a case in the Workhouse, upwards of 16 years ago, in which *tetanic spasms and catchings, with cold clammy skin, intermitting pulse, and extreme general distress*, were complicated with deep-seated inflammation about the parotid, ultimately ending in very extensive suppuration, itself apparently the consequence of a sudden suppression of the menses, from the patient's having been much terrified, and exposed to cold, owing to a cry of fire after she had gone to bed, so that she rushed into the street, without her clothes, on a wet night. In this case, I was not deterred by the inflammation from *combining wine with large doses of drastics*, and with the best effect. The patient, a spare and apparently respectable woman, took a full quart of Port in twenty-four hours, and a gradually reduced quantity for many successive days. *A fortiori*, then, Does it seem expedient that, in the present instance, *wine*, or perhaps *brandy*, *to a considerable extent*, should have been exhibited along with the drastics? And I now regret exceedingly that more prompt attention was not paid to this very important point; but really there was not much time for consideration; the patient was dead in less than twenty-seven hours from the time of my first seeing him; and perhaps even then it was too late to have been of much use, for *the pulse was up*. I have already stated, that this symptom did not *immediately* alarm me as being more dangerous than *irregularity*, and in all other respects the case seemed comparatively mild; but next morning, when I found him with a pulse at 144, *though still without any remarkable increase of the tetanic affection*, my apprehensions were all excited, and I immediately directed the wine to be given more freely; but I know not whether he lived long enough even to make the trial.

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* Indeed it is so stated in the original manuscript from which I drew up my own remarks; but he presumes it must have been in the hurry of transcribing that it has been overlooked. He also wishes me to notice *one* typographical error, which obscures the sense. His latest Report is dated *since*: it ought to have been *June*. Here I might notice one or two trivial errors in my own Remarks; but it is scarcely worth while; the reader will have perceived them.

It was immediately after this last visit that I went to consult Dr Parry's work, in which he lays such particular stress on the *quickness* or *slowness* of the pulse, as the criterion by which, as he says, " we may fairly estimate the danger on the one hand, or the probability of success on the other." On this point he delivers his doctrines without much hesitation. " It will, I greatly fear," says he, " be found, that the boasted success of means for the cure of tetanus depends on the circumstances I have just stated, that is, if *the pulse remains quiet for the first five or six days*, the patient will recover under *any*, and possibly, in many cases, *without* any remedies. If it is *early quick*, he will die in spite of all known remedies." In another place, he says that death " usually occurs in *four* days, and often within *forty-eight hours* from *the first symptoms of the disease.*" Now, with all imaginable respect for this able physician, I would take leave to say, that, in what he has written on tetanus, he generalizes too much, and lays down rules and limitations to which Nature will not be bound. From *what period* are the " first symptoms" to be dated? In this last case, the patient was never free from *aching and stiffness in his jaws and limbs*, after he had lain for some hours on the wet ground; but it was not till more than a week afterwards that he applied for medical advice; and, in the mean time, he had been working and drinking, and had, on the fifth day, walked eighteen miles to a horse-race, and worked all the next day. Nay, even *after* he had applied, Mr Nesbitt states, that for the first four or five days the pulse remained quiet; and indeed, from the circumstance of his having had an uninterrupted sleep of three hours on the night of Friday, as also of his feeling so comfortable during Saturday, that, when the evening came, he had taken but one powder in twenty-four hours, one cannot conceive that, *up to that time*, much was wrong about the *heart.*

When once, however, the functions of this important organ are involved, I admit the danger is fearfully increased, and I readily subscribe to Dr Parry's proposition, that " whatever may be the mode in which it is acted on, the *heart*, in reality, seems to be that part of the frame which, in tetanus, first loses its vital powers." He goes on however to say, " If, in an adult, the pulse, by the *fourth* or *fifth* day, does not reach 100 or 110 beats in a minute, *I believe the patient almost always recovers.*"

N. B. Here we have a day less; though, *in the same page*, he speaks of *five* or *six* days; yet a day may make all the difference; for instance, had I seen Mr Nesbitt's patient on the Saturday morning, and *had done nothing*, I might have said, on the authority of Dr Parry, that as he had got over the fifth or sixth day (or, more correctly, the eleventh or twelfth), "without the pulse having reached 100 or 110 beats in a minute," he was almost sure to recover, with *any*, or possibly *without* any, remedies! Not only, however, is the time at which the pulse may become frequent, uncertain, but neither is the *converse of the proposition* to be depended on, namely, that when the pulse is *not quick*, the patient will recover; for in the case quoted from Stoll, in the Number of this Journal for April 1810, where the patient died in convulsions, "the pulse was *small*, scarcely *perceptible*, but *not quick*." It was idiopathic tetanus too.

Again, I cannot but think it rather a hasty assumption, that *all remedies* are mere matters of *indifference*. On the contrary, I should say, that such immense doses of drastic purgatives as Gaskell and Wright took, must have done *either good or hurt*. If they were not proper, they were enough to have *killed* the patients; and let the facts speak for themselves.

A little farther on, Dr P. dwells on the distinction between the *traumatic* and *idiopathic* species, the latter of which he is disposed to think rather lightly of, "as being disorders which have little tendency to a fatal termination. He who cures them," says he, "will scarcely *arrogate to himself* the merit of having cured tetanus." And such, I well remember, were the objections urged to me by some, when Gaskell's case was first published; (whereas, in truth, I had always expressed my doubts whether it was really traumatic or not; and my fears lest the practice, which had answered so well in it, might not have the same effect in a more decided case; only, as I said at the time, "should a less doubtful case occur to me, bearing in mind the frequent failure of every known plan, and the fortunate issue of this, I should think myself justified in giving it the same full trial.") But, without much of *arrogance*, it may be affirmed, that no such objection applies to Wright's case, for it was indisputably traumatic; and herein consists its greatest value, as setting that question at rest. Were I, however, to speak from my own limited observation and inquiries, I should say that the idiopathic species has proved the more intractable of the two. I have just adverted to the fatal case quoted from

Stoll; and I may add, that Dr Parry has himself recorded "*a case without a wound, which terminated fatally.*" It is true the patient lived at such a distance, that Dr Parry could not see him regularly; but then, if such "disorders have little tendency to a fatal termination"—if they be of that description which will "recover under *any*, or possibly *without* any remedies," this circumstance could have made little or no difference. For myself, I may be allowed to doubt whether the result might not have been different, if he had but given a full trial to those "large doses, frequently repeated, of calomel and jalap," which, he says, "he should be disposed to try, if another instance were to occur to him." It would still have remained to be inquired, what he would have *called* large doses; but I feel assured, that if once he had made up his mind to "*procure very copious evacuations during several days,*" the physician who had nerve enough to take his station in the front rank of those who first ventured *to bleed in purpura*, would not have been deterred, by light considerations, from following up his own convictions. If I may quote an observation of my own, I am persuaded he would have felt, that "if a remedy be indicated at all, the *dose* should be regulated, not merely by weight and measure, but by the *effect;*" and I am sure he could not have been more astonished at the *lengths* to which he might thus have found himself led, than I was, when I first learnt the unprecedented rate at which Gaskell was taking strong medicines (60 or 70 grains of calomel, with a pint of the infusion, containing jalap ℥ss. in the twenty-four hours.) It will be remembered, that on the fourth day (it was but the *third* of such active practice), I shrunk from the responsibility of pursuing an unknown path, and, as I now believe, I had nearly lost my patient; but, fortunately for him, I recovered myself in good time, and all went on well. The pulse had been, nearly from the first of my attendance, at 100 or 104. I question whether, *at the same period*, it was so high in Mr Nesbitt's patient; yet it ultimately became very rapid, then he died.

But again, Dr Parry makes a distinction between *acute* and *chronic* tetanus; and, alluding to some cases published by the late Dr Rush, to recommend *wine* and *bark* in large doses, he says, that "since these cases have been defective in that precise enumeration of symptoms" (query, which be they?) "which would enable one to judge whether they were of the *chronic* or *acute* kind, we have no means of appreciating the proposed remedies. On the whole," he adds, "it appears to me, that the *cure of acute tetanus is still to be sought.*

According to this doctrine, then, every case that has *not* *proved fatal* must at once be set down as *not* having been acute; otherwise I should have thought that Wright's had a tolerably fair claim to the title. It was *traumatic;* it came on in one night, after having *walked* eighteen miles during the day. And when, at the end of five days, he was brought home and placed under Mr Manifold's care, he describes him as "a most pitiable object, with his pulse at 105, universal stiffness, and frequent spasms, sometimes pulling him violently backwards, at others forwards, so that he falls from his chair, often with great violence; *difficulty of swallowing*, in consequence, as he says, of the spasm *attacking his heart;* much pain at the scrobiculus cordis, and down the back"—yet this boy recovered most rapidly: whereas the subject of the present remarks, it will be remembered, so far from being "such a pitiable object," at the same period after he first felt the disorder coming on, actually *walked*, on the fifth day, as far as Wright had done before he was attacked, and got tipsy at night, and worked at his trade the whole of the next day, and did not ask advice till the end of the second day after all this; and it was not till after *eight* days farther that he died; making, in all, a period of fifteen or sixteen days.

I confess, therefore, I do not see either the grounds or the utility of Dr Parry's distinction. Certainly, where the disorder *runs a fatal course very rapidly*, it may well be termed *acute*. I have myself seen such a one; it was of the genuine traumatic species, in a man; he was properly the patient of my friend (and at the time colleague in the Dispensary) Dr Jeffreys, who saw him but once; and being obliged to leave home on the evening of the same day, had desired *me* to visit him. I found him with his jaw very completely locked; but he had been driving his cart only a few days before, and was then sitting with his family at a tea-table, in his ordinary clothes, and spoke quite cheerfully; but, as I afterwards learnt, I had scarcely got a hundred yards or two from the door, when he was carried off, probably by a *spasm of the heart*. But here again, neither does the converse always hold good, nor is the *mortality* at all in proportion to the *acuteness*. Witness the two cases last cited. But farther, in my remarks appended to Gaskell's case, I have adverted to one of trismus, which had occurred at the Dispensary to my friend and colleague Dr Carson, only a few days after mine, and during the extremely hot weather of which I spoke. Of this case I would say, that if any, it was entitled to the appellation of *chronic;* and I would quote it, as a striking illustration *both ways*, of the *efficacy* of drastics on the one hand, and of the fatal consequences of *neglecting* them on the other.

The patient had received so much benefit from Dr Carson, that he had ceased to apply, or to take any medicines, but he never got quite well; and, in the absence of Dr Carson, at the distance of *more than five weeks*, *I* was desired to visit him. Of course I merely recommended a repetition of his former medicines; but these could not be administered without great difficulty and firmness. He was an orphan boy, much indulged, and very refractory. Nothing was done, and he was carried off in a convulsion.

Besides, it remains to be proved, that in cases which may have appeared the *most acute*, there did not exist, at one time, *before* the functions of the heart were materially deranged, a state of things, of which, if due advantage had been taken to establish a counter irritation, the disorder might not have been arrested in its progress. To take, for example, the case of the boy which Dr Parry himself quotes, "the parts principally affected were the dorsal muscles, a power remaining of opening his mouth to a certain degree, and of swallowing a little. On the first day there was only a *slight stiffness*, *with a natural pulse*; on the second day the pulse was 86; on the third day 96; on the fourth 120; on the fifth 160, when the patient died." Now, I would ask whether, if this boy had but been treated *from the first* as Wright was, though he did not apply till the disease had already gained such strong hold, and the pulse had got up to 105, the result might not have been equally favourable, for *aught that appears to the contrary?* At any rate, the *negative* must not be assumed. In this view, it is important to observe how the *pulse gradually came down* in Wright's case, from the time that he was put *under active treatment*, while yet all other symptoms appeared to be on the *increase*; so that on the fifth day after he had come under Mr Manifold's care, "when he had a very bad night, with a continuity of spasm from 12 till 7, and the rigidity so very great, that his parents thought he was dying. *His pulse was at* 84, and he complained more of pains in his *groins* and *belly*, but not in the least *of pain at the scrobiculus cordis*;" thus verifying, almost to the letter, what I had said at the time, as to the scene of conflict being removed from the citadel to the outworks. But for a farther illustration of the mysterious connexion between the functions of the heart and those of the alimentary tube, I would refer to the reports for the 23d, which was indeed the day of the *decisive* conflict; always remembering that he had taken, in the course of three hours, in the morning, almost *twice as much as is stated in the printed report*. It will be seen that, so late as 7 *p. m.*, before he took the croton, he appeared to be worse in every respect; "the pulse was again at 105, *small and weak*;" but only two hours afterwards, when relief had been obtained, *it had settled to* 72; and from that time, it never rose more than two or three strokes in a minute.

As to the cases published by Dr Rush, not having seen them, all I can say is, that it was on their authority that Dr Currie and Mr Park gave such large quantities of wine, *and with such success too*; and it will hardly be contended, on any other principle, than *that it did not prove fatal*, that this was not an *acute* case; for, besides that it was *traumatic*, it is expressly stated that "the progress had been very *rapid*; the jaw rigidly contracted; the spasms extended to the neck and back; the pain under the ensiform cartilage was *most acute*: and twice or thrice, in every hour, he was seized with general convulsions, which lasted half a minute."

Now, it is not to be supposed that Dr Parry had *never read* Dr Currie's reports; but surely he had forgotten them, when he wrote as he has done respecting "the *precise enumeration of symptoms*," &c. I cannot therefore but regret that this excellent physician, instead of lending his great name to the establishing of such hypothetical distinctions, had not limited himself to his more general proposition, or that he had rendered it still *more* general, by saying, that in tetanus, *whencesoever arising*, or at *whatever period* of the disorder, whether sooner or later, when once the functions of the heart are *in any way* materially deranged, the danger to life is very much increased; and this I fear is undeniable. Possibly, too, the *quickness of the pulse* may be a more infallible sign of danger than *weakness* or *irregularity*, and I really believe it is so; yet Stoll's patient died with a pulse *not* quick but weak; and I think whoever had seen my patient in the workhouse, *when she was at the worst*, would have been apt to believe that she was in fully as much danger of a convulsion as he was.

Indeed I would willingly cling to this case, as a proof of the possibility of warding off a fatal blow, under very discouraging circumstances; for my sincere belief is, that if ever I rescued a patient from imminent danger in my life, it was this very one, as decidedly as ever I saw one rescued from *Cholera* or *Croup*; and this, be it remembered, was effected by the *free exhibition of wine, combined with large and repeated doses of drastic purgatives*: and I would suggest, that much seems to depend on the *combination* of the two, as rendering it unnecessary or even injurious to give *wine* in such quantities, or for *such a length of time*, as when it is trusted to singly. In my remarks originally subjoined to Gaskell's case, I took this view of the question, and later experience has rather strengthened it. Gaskell himself could not, at the most, bear more than a bottle of Sherry in 24 hours, and took but thirteen bottles in four weeks; at the end of which, he *walked* to the Dispensary, and soon got quite well, and has remained so. My *female* patient in the Workhouse, took but nine quarts in 16 days, when it was discontinued. *She* too got quite well, but not till after a relapse, from which she was recovered by a *bleeding*, and drastics as before, *without* wine. As to Wright, it will be recollected, that he took *very little* wine, and that very soon it was found expedient to discontinue it altogether. And yet he was nearly well in a week from the time when he was at the worst: whereas, in Dr Currie's patient, "for a long time life and death seemed to hang in equal scales; nor was it till he had been 42 days under treatment, that his safety could be ascertained, and during this time he had swallowed 110 bottles of Port wine." How much longer it was before he was *dismissed cured*, is not stated, but it seems that, at the end of *ten years*, "his features retained the indelible impression of the disease."

I have already admitted that, in the present instance, I may have erred by laying *too little* stress on one of these indications, though *not too much on the other;* but after the best consideration that I am able to give to the subject, I confess I cannot suggest any means for the treatment of tetanus, *in all its stages*, which promise to be more generally successful than this same combination; taking for granted, of course, that in each separate case, the practitioner, will use his own discretion, as to quantity and proportion, according to the constitution and previous habits of the individual; always remembering, however, how much depends on *promptitude*, that the keeping of the vital spark alive, *even for a few hours*, may make all the difference between life and death.

Sometimes it has occurred to me as a question, how far the above principles would apply to hydrophobia; in fact, the same question has, I believe, been suggested by Dr Hamilton. In such cases indeed, as those detailed by my friend Dr Brandreth, in two recent Numbers of this Journal, I confess they seem wholly *inapplicable ;* and considering the appearances on dissection in those two cases, I do not see what could have been effected by any human means. If ever there was a period, in either of them, when any good could have been done, that period I conceive was gone by, *before* they were seen by Dr Brandreth. But it may serve to show the uncertainty in which every thing relating to this frightful disorder is involved, when it is recollected not only that in these two cases, the parts affected *were different;* but in another case, owing to the bite of a *rabid cat*, which occurred soon after the others, and which I saw, there was *no appearance of inflammation in any part:* so that in this case and some others which I remember to have read, I should say that, unless there be something in the *nature of the specific virus*, which will for ever baffle all human efforts, judging merely from the symptoms and dissection, it seemed within *the limits of possibility* that, by prompt and vigorous measures, something might have been accomplished in the way of *counter irritation.* I am aware of the great additional and almost peculiar obstacle to the exhibition of cordials in this disorder, arising from the utter impossibility of swallowing a liquid, which the wretched patients in general labour under. In the last case which I saw (that owing to the bite of a cat) it was complete; the very *sight* of a liquid would bring on a paroxysm: but this is not universal; for instance, in the case of poor Mr N. (Dr Brandreth's latter patient). I am sure I have seen it to a greater degree in some mixed cases which seemed to partake of the several characters of hysteria, hydrophobia, catalepsy, and tetanus: and even the poor woman whom I saw, managed to get down a *dose or two* of calomel and scammony mixed, with preserved currants; but the plan was superseded for a copious bleeding and large doses of liquor plumbi. Still as she *did* get down this last, it seems possible that small quantities of brandy, disguised in soft sugar, might have been swallowed instead. Perhaps *musk* might have had a good effect. At any rate, it is not claiming *very much* for such practice, to say that it promises *quite as fairly* as any thing else that has been proposed. Only that here, perhaps more than in any other case, whatever is done must be done quickly.

(To be continued.)

VIII.

On the Maternal Fœtal Circulation. By David Williams, M. D., Liverpool.

The inquiry relative to the nature of the medium, and the way by which the fœtus of the viviparous animal is nourished in utero, has engaged the attention of the philosopher from an early period. Of the doctrines which have been entertained on the subject, none, however, as yet, has been supported by conclusive observations, although the modern authors, whose writings I am acquainted with, appear to be unanimous in giving their approbation to an ingenious hypothesis, to which we shall have occasion to refer hereafter.

The communication between the viviparous animal and its offspring in the womb, is effected in the secundines by two different orders of arteries and veins, and the placenta is generally described as being apparently composed of those vessels, blended either with cellular substance, or perhaps some peculiar, and not yet ascertained tissue. One order, the fœtal system of vessels of the placenta, is a continuation of those of the funis or umbilical cord; the other, the maternal or uterine system of vessels, arises from the vessels of the uterus. Equivalents in some respects to the secundines of the viviparous animal, are observed to be connected with the embryon of every organized production of nature, and in the ovum or seed of the oviparous species, the embryon and secundines communicate with each other by vessels, in like manner as in the viviparous species, but under many modifications. In those classes which exclude their young in the ovum, the secundines seem to be intended for two purposes;—one, to be a reservoir of pabulum, suited to the nourishment of the embryon for a limited period after its vital principle has been excited into a new action;—the other, the effecting a change in the circulating fluid of the embryon, analogous to that which it undergoes in the organs of respiration of its own adult species. Experiments and observations on the discotyledons, or plants whose seeds are furnished with two placentæ or cotyledons, prove the principal part at least of the nourishment of the incipient vegetation, to be derived from the pabulum deposited originally in the cotyledons; and also its aeration to be carried on through the vessels of the latter. Thus during germination, the vegetable is nourished by the cotyledons, until its roots and leaves are sufficiently matured to perform their adult function. Then the communication ceases between the cotyledons and the infant plant, for the same reason that the communication ceases between the secundines, and the fœtus and uterus in the viviparous class at the time of parturition. Experiments and observations likewise demonstrate, that the vitellus and albumen of the ovum of the fowl, contribute, during incubation, to the nutrition and growth of the chick, and that the exterior membranes which envelope them act as organs of aeration.

Notwithstanding our intimacy with the channels and fountains which supply the embryon of the fowl and vegetable with food and air, our information continues to be extremely defective, relative to the nature of the immediate mode of communication which exists between the vessels of the fœtus of the viviparous species and the sources of its nutrition. The absence in the uterus of reservoirs of pabulum adequate to the support and increase of the fœtus during gestation; the connexion of the uterus with the secundines, and the latter with the fœtus; the fatal effects which speedily follow the compression of the umbilical cord, whilst the offspring is *in utero;* and also, the consideration that the fœtus itself is incapable of creating materials for its own increment, have induced philosophers to infer, that the fœtus of the viviparous animal must needs receive its nutrition from the system of the parent, according to its present exigency. Though all have acquiesced in this opinion, yet different hypotheses have been entertained by different individuals respecting the quality of the nutrition which passes from the uterine system of vessels into the fœtal.

Many physiologists have supposed the blood itself to be circulated between the vessels of the uterine system, and the vessels of the umbilical cord. Hieronymus Fabricius was one of those who assented to this opinion. He imagined that the veins conveyed the blood, and the arteries the spirits (*spiritus vitalis, seu calor cordis*), from the system of the uterus into the system of the fœtus, uninterruptedly, and by continuity of canal. So, agreeably to the popular philosophy of the times, Fabricius fancied that the fœtus received its blood and spirits directly from the liver and heart of the parent. * Julius Cæsar Arantius, a contemporary of Fabricius, agreed with those who maintained a different notion. He supposed the nutriment (*humor*) and spirits to be imbibed by the small orifices of the extreme ramifications of the umbilical vessels, from the uterine system of the placenta. † These were the prevailing doctrines of the day, when our illustrious countryman made the grand discovery of the greater circulation. The author of the important physiological revolution, approved of, and zealously supported the main principle of the hypothesis subscribed to by Arantius; and he expressed his sentiments very explictly, both on the nature of what he conceived the circulating medium to be, and on the structure forming the communication between the vessels of the fœtus and those of the uterus. Dr Harvey says, that he thinks the placenta to be appropriated to the secreting of pabulum for the nourishment of the fœtus, and he compares its function to that of the liver and mammæ. "Jecur, inquam, est organum nutritivum corporis, in quo est; mamma, infantis; placenta, embryonis." * In examining the doctrine of inosculation of the maternal and fœtal systems of vessels entertained by Fabricius ab aquapendente, Dr Harvey remarks, that there can be no greater reason for supposing the umbilical and uterine vessels anastomosing one with the other, than the sanguiferous vessels of the mammæ anastomosing with the excretory ducts of the lacteal glands. Also, in speaking of the cotyledons or placentulæ of glanduliferous animals, he has these words: " Cavitates istæ, spongiæ majoris loculamenta magnitudinæ non excedunt; usque singulas earum, totidem vasorum umbilicalium ramuli tenuissimi profunde penetrant: quippe in iisdem alimentum fœtui reconditur; non quidem *sanguineum* (ut Fabricius voluit), sed *mucosum*, ovique albumen crassius plane referens. Unde etiam manifestum est (ut dudum diximus) bisulcorum fœtus (ut et alios omnes) sanguine materno non ali. " †

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* De formato fœtu, pars secund. cap. De vasorum umbilical. utilitatibus.

† De humano fœtu. cap. De generatione et usu uterini jecoris.

In consequence of an Essay on the Nutrition of the Fœtus in Utero, by Mr Gibson of Leith, ‡ who defended the doctrine of the fœtus being nourished both by the mouth and by the vessels of the umbilical cord, the attention of Professor Alexander Monro *primus* was particularly called to our subject. In an Essay, in reply to Mr Gibson, by the Professor, we have quotations from several experiments that have been published; in which it is asserted by their respective authors, that injections had passed from the uterine arteries into the umbilical vessels. As I have not been able to procure a perusal of the original works, in which the experiments are detailed, and as Dr Monro has brought forward very material observations in discussing their merits, I have taken the liberty of quoting the following information from his interesting production. § The first experiment alluded to by Dr Monro, is reported by Dr Drake, in his Anthropologia Nova, as having been executed by Mr William Cowper. Dr Drake says, " Mr Cowper kept by him of a cotyledon and part of a uterus of a cow, in which mercury poured into a branch of the uterine artery, went into one of the cotyledons of the uterus, and filled those branches of the umbilical veins which went from the cotyledon to the navel of the fœtus. " *Secondly*, Dr Noortwyk, in his Uteri Humani Gravidi Anatomia et Historia, affirms, in his description of the dissection of an injected impregnated human uterus, that his injection passed from the uterine arteries into the vessels of the chorion and placenta. The force of this assertion, however, is entirely done away with; for Professor Monro makes it quite evident, that Dr Noortwyk committed an error, in mistaking for the placenta the cellular substance situated between the muscular coat of the uterus and the villous membrane which forms its internal covering. In the same narration, Dr Noortwyk also says, that " the injection into the impregnated womb did not penetrate into the fœtus or umbilical rope. " And again, he says, " On the side of the placenta farthest from the womb, the red colour of the injection shined through the chorion; and in one part, where the chorion was taken away, the small injected vessels were to be seen in the preparation. " *Thirdly*, " Slade is quoted by Blasius for saying "—" The liquor injected into the arterial vessels of the uterus (of a cow), was carried to the cotyledons, and by the cavities of the cotyledons into the substance of the placentæ. " *Fourthly*, and *lastly*, Dr Monro quotes Dr Raymond Vieussens, who is said to have made the following experiment. " He tied the left carotid artery of a living bitch with young; and then, having put a small ivory funnel into the right carotid, he poured quicksilver at different times towards the head, till it amounted to about four pounds. By the time this quicksilver was poured in, the creature appeared to be quite dead, and he dissected her before a great many witnesses. " After describing the progress which the quicksilver had made in the vessels of the bitch, he says, " That fluid (the mercury), without breaking any vessel, or the effusion of one drop of blood, passed through the placentæ surrounding each whelp, and was pushed into the umbilical veins themselves. " In another treatise, he (Dr Vieussens) gives an account of this experiment in these words: " Mercury being poured into the right carotid artery of a bitch about two months gone with whelp, the left carotid artery being tied, passed into the umbilical vein of the whelps, without any breaking of the vessels. "

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* Exercitationes de Generatione Animalium. De Placenta.

† Exercitatione de Generatione Animalium. De Acetabulis.

‡ Edinburgh Medical Essays. Vol. I. Art. XIII.

§ The Works of A. Monro, M.D. Essay on the Nutrition of Fœtuses.

The subjects of Cowper, Noortwyk, and Slade, had evidently been dead for some time previous to their being injected; consequently, the irritability of their bloodvessels must have been so far exhausted, as to have been incapable of rendering any assistance in the circulation of the injected fluids. As they were placed in that respect under similar circumstances, we shall give them a general consideration, by principally quoting a few of Dr Monro's experiments and observations. " Drake, " says the Professor, " expresses himself so little like an anatomist, in comprehending both the glandula of the womb, and the placenta of the secundines, under the name of *cotyledon*, that I suspected his committing a mistake; therefore, I repeated the experiment many times, by pouring mercury sometimes into a branch of the uterine arteries distributed to one of the glandulæ, and at other times, I poured the mercury into a branch of one of the umbilical arteries sent to a placenta, but never could make one drop of it go from the vessels of the one into the vessels of the other." In another place the Professor tells us, "Having fixed a pipe into one of the iliac arteries, and having tied the other iliac artery and the veins, I pushed through the pipe fine oil of turpentine. I continued this injection till all the vessels of the womb were in hazard of bursting. Not one drop of this oil was found in any branch of the umbilical vessels, or in the fœtus." Professor Monro, in concluding his observations on the experiments attributed to Cowper and Slade, says, "I have tried injections of very different kinds so often into the vessels of the womb and secundines of cows, prepared in all the different ways I could contrive for making liquors pass from the one to the other, without having once made a drop to pass, that I cannot be more certain of any thing, than that there is no anastomoses or continuity of these vessels in cows." The shining of the injection through the chorion and amnios, as remarked by Dr Noortwyk, is a common occurrence. For, when we inject from the uterine arteries, we render a portion of the placenta turgid, by forcing the injection into its cells and fissures.

We have next to examine the assertion of Dr Raymond Vieussens, respecting the result of his strangely conducted experiment. Although Dr Vieussens's subject was alive at the commencement of the experiment, yet, from the manner in which he conducted it, the sensibility of the animal must have been extinguished for some time previous to the quicksilver reaching the uterine system of vessels. The advantage, therefore, that Vieussens derived from the irritability of the vessels in circulating his injection, can be equally as well insured by the forwarding of the mercury into the uterine vessels, the instant the animal is destroyed, through a more direct channel than the one he selected. Dr Monro says, "I endeavoured to imitate Vieussens's experiment on a living bitch, but the creature died before any success could be expected; and therefore, with the assistance of my colleague Dr Andrew Sinclair, P. M., and of Mr Gibson, I took another way to try if the mercury would pass from the womb into the umbilical vessels." Then Dr Monro gives us an account of two pregnant bitches, into whose carotid arteries he introduced quicksilver in the direction of the aorta immediately after their destruction, at the same time suspending them by the necks, (above where the pipes were inserted into the vessels), so as to give the pressure of a high column. In describing the appearances of one of them on dissection, he says, "After the amnios was opened, there was no mercury to be seen in the fœtus, or in the umbilical vessels, though we (meaning Professor Sinclair, Mr Gibson, and himself) could trace these to their very minute branches in the placenta and membranes."

To make it yet more satisfactory to myself, as to whether quicksilver would pass from the uterine into the umbilical vessels, while the irritability of the system was still active, a bitch that had gone about two months with young was pithed, and immediately quicksilver was introduced into the descending aorta. The animal was suspended by the neck. After continuing the process until the hind feet were pretty tense with the mineral, the uterus was opened, and the fœtus were carefully examined. Not a globule of quicksilver could be detected in their systems, or in any of the branches of the umbilical vessels. When the placentæ were separated from the uterus, a quantity of mercury escaped from the vessels of the latter. In one of the placentæ which had been slit open, a globule of quicksilver was observed shining below the amnios and chorion. From its bulk, it was evident it was not within the coats of a blood-vessel, which was proved to be the case; for, on gently opening the exterior or uterine surface of the placenta, above the apparent lodgement, and inverting it, the globule dropped out. For the purpose of seeing whether the irritability of the blood-vessels was equal to the propelling of mercury against gravity, another animal was destroyed. Instantly after tying its trachea, four pounds of quicksilver were gradually introduced into the left carotid, (the head being suspended over the edge of the table), and, in imitation of Vieussens's experiment, a ligature was passed round the right carotid. When about one half of the mercury had been poured into the artery, it began to escape at the nostrils. On examination, it was found to have ruptured the vessels of one of the eyes, and to have insinuated itself into its chambers. None of the mercury could be detected in the vessels of the thorax and abdomen, while the head remained suspended. On raising the head above the level of the body, the quicksilver rapidly penetrated into every part of the system. From this experiment mercury would seem to be too heavy a substance for the irritability of the blood-vessels (immediately after the destruction of an animal) to circulate against gravity. The confused description given by Raymond Vieussens, of the progress made by the mercury along the umbilical vessels, inclines me to think, as it did Dr Munro, that he must have fancied the mercury, worked into the fissures of the placenta, (as the secundines were handled,) to have been contained within the coats of the branches of the umbilical vessels. The hypothesis approved of by Dr Vieussens seems to throw a doubt of his own firm conviction of the mercury having entered into the umbilical vein. If he had really believed that the mercury had circulated from the uterine vessels into the umbilical, would he not naturally have maintained that red globules of blood were probably circulated to and fro, between the maternal and fœtal systems? However, instead of being the advocate for a doctrine of this kind, we find Dr Vieussens giving his approbation to the opposite hypothesis.

From the above statements, and from the sentiments expressed by the authors I have perused, it appears to me, that neither mercury, nor any subtile fluid that is used as an injection, can be forced from the maternal into the fœtal vessels, and *vice versa*, after the irritability of the blood-vessels has become extinct. Also it appears, that although we avail ourselves of the irritability of the blood-vessels the moment an animal has been deprived of sensibility, yet we cannot introduce quicksilver from the sanguiferous system of the parent into that of the fœtus.

The vain efforts of Dr Monro and other anatomists, in their repeated endeavours to inject the fœtal system of vessels from the uterine, brought the speculation of the blood being the circulating medium into disbelief. In process of time, the failure, by passive consent, came to be admitted to be one of the principal proofs of the formation and circulation of the blood of the fœtus being independent of the parent, excepting that the materials for its fabrication could only be derived from the system of the latter. When the influence of the atmosphere on the blood in the adult lungs, and the action of the same agent on the blood of the chick *in ovo*, and on the fluids of the embryo vegetable, became to be duly appreciated, it was conjectured that the blood of the fœtus must also be exposed by some means to the influence of the same vivifying element. As the fœtal blood, according to the prevailing theory, could have no immediate access (such as the blood has in the adult lung) to the atmosphere, it was hinted by Sir Edward Hulse, * that the placenta might be an organ of respiration or oxygenation, as well as an organ of nutrition. This idea by degrees gained ground, and at present the placenta is considered as appropriated to the secreting of *pabulum nutritionis*, and to the executing the office of a lung. The blood of the fœtus, when conveyed into the substance of the placenta, is imagined to be there exposed to the influence of the maternal blood, which is thought to be so charged with the vivifying principle of the atmosphere, as to effect on the fœtal blood an analogous change to that which it undergoes in the lungs after birth. There is something extremely plausible and fascinating in this hypothesis, yet the data which have given rise to it appear to me to be deduced from analogies and observations of too ambiguous a nature, to sustain satisfactorily the inferences drawn.

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* Study of Medicine. Fifth Physiological Proem.

While reflecting on the power which I had observed the blood-vessels to possess, of circulating oily fluids for a time after an animal was destroyed, and also on the obstruction to the circulation in the lungs, which takes place on the stoppage of respiration,* they appeared to me to present a clue by which the nature of the communication between parent and fœtus might be disclosed. The following investigation then was undertaken, from a preconceived idea that the *vis insita* of the blood-vessels might mainly contribute to the establishing, by experimental observation, one of the physiological speculations we have been reviewing above. To give an insight of my preconceived views, I shall briefly mention the phenomena which I thought would necessarily result from, and consequently indicate, each mode of communication, on injecting oil into the maternal arteries, immediately after the destruction of an impregnated animal. First, it was presumed if the communication should be carried on between the fœtal and maternal vessels uninterruptedly, that the injection would permeate the vessels of the fœtus in the form injected into the system of the parent. Secondly, if the communication should be through a secerning system, (secreting *pabulum nutritionis*), that the injection would not pervade the fœtal system of vessels under the form introduced into the vessels of the parent. Thirdly, that if the placenta, in addition to the last presumed function, performed also the function of a lung, then, besides the exclusion of the injected fluid from the fœtal system, it was imagined that an obstruction and engorgement would take place in the umbilical arteries, as oil reached the maternal portion of the placenta. For, as an obstruction and engorgement takes place in the pulmonary artery when respiration is stopped, from a deficiency (in the atmosphere pent up in the lungs) of the principle necessary to the decarbonization of the blood; so, on the same principle, it was conceived that a similar phenomenon in the umbilical arteries would be the result of the presence of oil, instead of the vital stream in the uterine system of vessels. The anastomoses of the umbilical arteries and veins were expected to lessen the engorgement; but, at the same time, it was thought that if the placenta performed the function of a lung, that the mode of communication between the extreme branches of the umbilical arteries and veins could not deviate greatly from that of the pulmonary vessels, therefore that a turgescence of the umbilical arteries would discover the presence of an obstruction in their system. These speculations were made known to my friend Dr Traill some months previous to putting them to the test of observation; and in that interval, with his usual candour, their merits were often discussed. I may truly say, from the important assistance I have have received from the same gentleman during the present inquiry, that few could be more advantageously associated in an investigation.

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* Edinburgh Med. and Surg. Journal, vol. xix. p. 526.

1*st Experiment.*—A bitch of a diminutive size, that had gone about six weeks with young, was destroyed by tying the trachea. Then an ivory pipe was introduced and secured in the descending aorta, within the cavity of the thorax; through which warm linseed oil, coloured with alkanet, was injected by a small common pewter syringe, with as little delay as possible. To prevent regurgitation during the refilling of the syringe, the orifice of the pipe within the artery was closed by the finger. After continuing to throw the oil in, until we thought it had traversed the system of the womb, Dr Traill and myself commenced the examination. On exposing the viscera of the abdomen, we found that the colour of the oil rendered our injection visible through the coats of arteries, as well as the veins; and from its being of a different tint to the blood, we could distinctly trace the extent of its progress along the vessels on the external surface of the uterus. The womb contained three pups. We extracted two of them, and then drained the vessels of the ends of the umbilical cords attached to the fœtus, upon white paper. A film of oil was distinctly seen on each of the drained portions of blood. Also, on making incisions into the bodies of the two fœtus, oil was apparent on the surface of the blood, which flowed from the divided vessels. The remaining pup was preserved.

2*d Experiment.*—To ascertain whether there was any peculiarity in the mode of communication between the above subject and her young, another of her own species, and about the same size, was pithed and injected. Films of oil were discovered on the blood received on paper from the vessels of the umbilical cords. Oil was also perceptibly seen on the surface of the blood, which escaped from incisions made in the bodies of several of the pups.

3d Experiment.—Dr Bostock and Dr Roget being in Liverpool, I had the pleasure of communicating to them the above experiments, and at the same time of expressing a desire to be favoured with their presence to witness a similar one; which invitation they kindly accepted. Our subject being of a large size and surly withal, it was deemed prudent to stun her by a blow on the head, before attempting to pith. Linseed oil, coloured with alkanet, was thrown into the descending aorta, by means of the apparatus already described. In consequence of Dr Traill and myself having repeatedly been able to trace the effect of the momentum given to the injection by the depression of the piston, as far as the extreme branches of the arteries (and no farther, the circulation in the veins being always uniform), an opening was made in the upper part of the abdomen, for Dr Bostock and Dr Roget to observe the like phenomenon. Contrary to my expectation, in this instance the impulse given to the oil at each ejection was but very feebly detected even in the larger branches of the mesenteric artery. This deficiency of power (in the impulse communicated by the discharge of the syringe) to produce shocks or pulsations in the minute arteries, arose partly from the apparatus being imperfect and too small for the size of the animal, and partly from the regurgitation of oil which took place, from the fingers of the assistant and my own being repeatedly seized with the cramp, while stopping the extremity of the pipe inserted in the aorta, at the time of the refilling of the syringe. It hence appears to be necessary, for the production of a visible shock in the terminating branches of an artery, that the quantity of liquid emitted, and the force applied to its ejection, should be in proportion to the calibre of the vessel; and likewise, that none of the injection should be allowed to escape during the process. After finishing injecting, the cells of the womb containing the pups were alternately opened, and the fœtuses extracted and separated from their secundines. The blood of each, as it issued out of the umbilical vessels, and from the incisions that were made in the bodies, was received on paper in several distinct portions. On examining these portions, we discovered films of oil only on a few of them. As oil might possibly have come in contact with the skins of the pups by extracting them out of the womb, and be the source of the few oily films observed, it was proposed, as an *experimentum crucis*, that two of the pups which yet remained without being incised, should be washed under the spout of a pump. By this precaution, we thought to evade the possibility of being deceived by oil that might be adhering to the external surfaces of the pups, spreading itself imperceptibly over the blood, which would issue out of the incisions that would be made in their bodies, after their ablution. The two fœtuses being washed, their internal cavities were laid open, and the large blood-vessels were divided, but no appearance of oil could be detected on the blood which oozed out.

4th Experiment.—Dr Traill present. Our subject measured sixteen inches from the occiput to the coccyx, and was so far advanced in pregnancy, that the superficial blood-vessels of the pups could not readily be observed on account of the development of the hair. Pithing was the mode of destruction. Afterwards, a stop-cock pipe was fixed in the descending thoracic aorta, through which a pint and a half of linseed oil, coloured with alkanet, at the temperature of about 100° of Fahrenheit, was thrown in by means of a three-ounce brass syringe. When half the oil had been injected, an opening was made in the right auricle of the heart, through which oil soon began to issue. It was supposed that this aperture would prevent an accumulation of blood in the cavities of the right side of the heart, and the large venous trunks; therefore, that the oil would be more freely circulated in the uterine vessels, as no stagnation could take place in them from an engorgement in the inferior cava. The pups were extracted, by making incisions across and through the parietes of the uterus into each cell, above the portion of the ovum, at either end, which extends beyond the annular placenta. After the opening was made, the uterus was grasped in such a manner, that the fœtus and its membranes were pressed towards the aperture, through which they were made to protrude beyond the cut edges of the uterus. The chorion and allantois were ruptured with hooks, then the amnion and fœtus were drawn out. This method was adopted as the one best calculated to reduce the chances of oil from the divided edges of the womb coming in contact with the fœtus. The first pup extracted had its funis secured. After washing it in different vessels with warm water, until no oil could be perceived on the surface of the latter, it was laid aside on a plate. Another nidus was exposed, the fœtus of which, after dividing the umbilical cord, was placed instantly to bleed in warm water. The action of the heart, which was feeble on its immersion, became so stimulated by the heat, as to project the blood out of the umbilical arteries to some distance, and to pulsate sixty-six times in a minute. Oil immediately began to make its appearance on the surface of the water, over which, by degrees, an oily film was spread, and ultimately, in several places, particles of oil congregated and formed extremely minute flakes. The first pup extracted, after having laid on the plate awhile, was put in a basin containing warm water, at the same time the funis was divided between the ligature and umbilicus. The action of the heart was not renewed, but the blood flowed from the vessels of the cord pretty freely, and oil gradually ascended to the surface. The third pup extracted had its funis tied, was washed, then after dividing its cord, between the knot and umbilicus, it was dropped into a vessel which held warm water; the vessels of the cord bled freely, and by degrees oil became visible on the water. The accumulation of oil on the water in the two latter vessels was not so considerable, as on that of the first. The fourth and fifth pups had their umbilical cords tied. From a deficiency of warm water, some time elapsed before they could be washed and placed in the vessels in which it was intended that they should bleed. When they were placed in them, no blood would issue out of the umbilical vessels; incisions were therefore made into the cavities of the chests, and the large venous trunks were punctured. From the one that was first operated on, a few pellicles of oil ascended to the surface, but from the last none appeared.

5th Experiment.—All our subjects having been pithed with the exception of the first, this trial was instituted for the purpose of ascertaining whether more oil could be introduced into the fœtal system by destroying the parent by securing the trachea, than by pithing. The animal was nearly of the same size as the one described in the last experiment, and about the same period of pregnancy. After we had injected, a fœtus was extracted, and the like method was observed with it, as with those of the fourth experiment. Oil ascended to the surface of the water, but Dr Traill and myself could not determine whether one of the modes of destruction alluded to, would be more favourable than the other to the introduction of oil from the maternal into the fœtal system.

My friend, Professor T. Thomson of Glasgow, did me the favour of transmitting the above experiments to the Royal Society, before which they were read.[*] Shortly after the reading of my paper, it was suggested, that it would be advisable, before committing it to the press, that a less equivocal mode of experimenting should be had recourse to. The objection raised against the validity of the foregoing experiments was, that the

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[†] See Annals of Philosophy for April 1825.

of the two pups that were washed under the spout of a pump. From the subject of this experiment having been but indifferently injected, and from the partial appearance of the oil, doubts may reasonably be entertained respecting the source of the oil on the portions on which it could be detected. In the fourth and sixth experiments, oil ascended to the surface of the water from every pup that was examined, excepting from the last pup in the fourth. In these two latter experiments, it will be noticed, that a marked diminution in the quantity of oil on the water, could be discovered the longer the bleeding of the fœtus was suspended; and after a time, it ceased to appear altogether. Could this gradual diminution and final disappearance, be attributed to the oil adhering more and more tenaciously to the sides of the bloodvessels as the circulation flagged? It is evident from our details, that in the most successful of our experiments, we introduced into the systems of the fœtus but a small quantity of oil in proportion to the blood which we found in them. The present state of our knowledge may be too imperfect to solve the reason of this partial introduction of oil only, yet before I conclude, I shall venture to hint at the causes which I conceive to have obstructed the entrance of a greater quantity. What could have been the reason in Dr Monro's trials and my own, of the non-admittance of quicksilver into the fœtal systems from the uterine, at a period that would have been favourable to the admission of oil? Did it arise from the quicksilver presenting a mechanical obstacle? Or, from its being obnoxious to the umbilical system?

From secretion being a function, the end of which is the formation of fluids which have no previous formal existence in the blood: the passage of the oil into the vessels of the fœtus in our experiments, would appear to be inconsistent with the idea of its having passed through a secerning system. This position granted, it is highly probable that the capacity of the communication between the *uteri* and *foetus* in the subjects of our experiments, had been capable of allowing the passage of the red particles of the blood. Accordingly, I am disposed to conclude the maternal and fœtal systems in the canine species, to be parts only of a common uninterrupted sanguiferous system. The analogous situation, in which the fœtus of viviparous animals in general are placed *in utero*, as to the influence of external physical agents, appears to me to declare very obviously, the mode of communication between parent and fœtus to be similar in all. If this analogical inference, and the previous conclusion be admitted, we shall have reason to doubt the validity of the doctrine of the maternal fœtal communication as taught by Dr Harvey, together with its modern superstructure. For if my experiments and deductions be correct, we can no longer subscribe to the hypothesis of there being two independent sanguiferous circulations in the impregnated state, and to the placenta being an organ of aeration.

Admitting the quality of the medium of communication between the uterine and umbilical vessels to be blood, it would seem to me, from the small portion of oil discovered in the fœtal systems in our experiments, and from the fœtus not being found deficient of blood when the parent is bled to death, that the maternal fœtal circulation must be obstructed, previous to either the maternal, or the fœtal vessels, becoming too debilitated to propel their contents. It is well known, that the fœtal system does not appear to be in the least drained of its blood when an impregnated animal is killed by the butcher. In this case it cannot be questioned, that the heart of the foetus continues its action, and that the irritability of the blood-vessels of the maternal and fœtal systems continues vigorous a while, after the left ventricle of the heart of the parent has ceased to propel blood to the uterine arteries. A cow far advanced with calf was slaughtered; presently, on examining the calf, it was found to contain a large quantity of blood, which issued out in a stream on dividing the jugular vessels. In this instance, the maternal fœtal circulation must have been obstructed soon after the cow had been "*stabbed,*" and while the irritability of the fœtal heart and of the maternal and fœtal vessels had been yet active. For the blood of the cow naturally rushing towards the point where there was the least resistance, the uterine vessels could only have replenished the system of the calf for a very limited period. Therefore, if the blood propelled to the placentulæ by the umbilical arteries had been forwarded to the uterine system, as long as it had been able to have conveyed it towards the inferior cava, the calf would not have had in its system, in my opinion, so large a portion of blood as that which escaped out of its jugular vessels. It would appear to me to be a law of the animal economy, that as soon as blood ceases to enter the umbilical vessels from the uterine, that blood also ceases to enter the uterine vessels from the umbilical. Thus the draining of the fœtus of blood, (in the event of the sudden death of the parent), would be prevented, and means would be gained towards its preservation for a time in the womb, independent of the maternal sanguiferous circulation. Likewise, as an additional means of preservation, the fœtal blood propelled to the placenta, (after the maternal fœtal communication has been cut off), would appear to pass from the umbilical arteries, through the anastomosing branches existing between them and the umbilical vein, into the latter, to be again returned into the fœtus. In this way, the fœtal circulation after the cessation of the maternal fœtal, might be carried on uninterruptedly as long as the irritability of the heart and vessels of the fœtus would be able to circulate the blood. Is it not this adjustment of means to ends that regulates occasionally in some animals the foetal circulation during parturition, from the instant the sanguiferous communication is cut off between the offspring and the parent, until the former respires? And, is it not to the above law and prospective contrivance, that the infant owes its preservation when its life is saved by the Cæsarian operation after the death of the mother? Pray, does any blood pass directly from the umbilical arteries into the umbilical vein, while yet the maternal fœtal circulation is going on uninterruptedly? If we accede to the affirmative of this interrogation, then we shall admit that a smaller quantity of blood enters the umbilical vein from the uterine system in a given time, than is returned to the placenta by the umbilical arteries from the fœtal system within the same period. Or in other words, that the blood which is conveyed by the umbilical vein to the fœtus is not *entirely* received *directly* from the uterine arteries, but partly from them, and partly from the umbilical arteries. This view, and the foregoing one which supposes an early obstruction of the maternal fœtal circulation, on the destruction of the parent, offer, in my opinion, a probable explanation why comparatively to the quantity of blood, so small a proportion of oil, could be found in the systems of the fœtus in our experiments.

Admitting that the uterine arteries supply *directly*, but a *portion only* of the blood returned to the fœtus by the umbilical vein, it may be a query, whether the fœtus of certain animals do not circulate blood that is more of the nature of arterial blood, than the fœtus of other species? If so, then the young animals that are the least capable of enduring the privation of air, and that have the greatest power of generating heat at birth, * may most likely be expected to be the more liberally supplied with blood *direct* (during the fœtal state) from the uterine arteries, and *vice versa.*

In injecting the fœtus from the maternal system, and in detecting the oil in the fœtal, I think attention to the following suggestions might be of advantage. The impregnated subject should be deprived of sensibility, by having the trachea tied at the acme of inspiration, so that the lungs may be in a state of full inflation after the chest is opened. This mode of destruction gives the operator time to remove away the sternum and the cartilaginous ends of the ribs; and also of passing a ligature under the aorta, while yet the circulation is going on uninterruptedly. After this preparative step has been taken, puncturing the aorta, and the inserting and securing a pipe in it, with the ligature already prepared, should be the next object. As soon as this has been accomplished, the injection should be thrown into the pipe *immediately*. On the speedy execution of this act mainly depends the success of the experiments. Therefore, the sooner the shock, caused by the depression of the piston, pervades the uterine system of vessels, after the communication has been cut off with the heart, the better. The injection should be emitted from the syringe, not in an even stream, but in reiterated, quick, and smart discharges. This method greatly accelerates the motion of the injected fluid in the arteries, and, consequently, its passage to the placenta. What would perhaps send the oil still more expeditiously towards the placentary system than the common syringe, though ever so expertly handled, from the time taken up in the refilling of it, would be an appropriate injecting apparatus, on the principle of Mr Reid's patent one for the stomach. For the same reason, such an apparatus would also be better adapted than the common syringe, for the purpose of exhibiting the nature of the progress of the blood in the artery and vein, as alluded to in third experiment. After the second syringe-full of oil has been injected, the right auricle should be punctured. This aperture will be a guide in directing when to desist injecting; perhaps, we should continue the process for two minutes, after the oil has commenced issuing out of it. This opening also, from its preventing engorgement in the vessels of the uterus, enables the oil to be circulated in them more freely. The moment we finish injecting, the examination of the fœtus should be entered upon without delay, in the manner described in the sixth experiment. The observance of this suggestion is of much importance. For, as the irritability of the sanguiferous system of the fœtus becomes more and more feeble, the presence of the oil on the water becomes less and less distinct, until at last none can be detected.

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* De l'Influence des Agens Physiques sur la Vie. Par W. F. Edwards.

I shall here observe, that experiments similar to those adopted in the above inquiry, are extremely well calculated to show the state of the blood-vessels of the stomach and small intestines, under the different alternate conditions of the alimentary canal. To demonstrate the vessels in the opposite extremes of these conditions, it would be necessary that animals should be examined, after being destroyed in the following states. The stomach and small intestines of one animal should be empty of aliment; another, after twenty-four hours of fasting, should be allowed a full meal of animal food, shortly before its death; and the lacteals of another should be actively engaged in conveying chyle. After injecting subjects with coloured oil, under these conditions of the alimentary canal, the blood-vessels ramifying on the stomach of the animal lately fed, would be found to be much more conspicuous and larger than the corresponding vessels of the one with the stomach empty. Likewise, the mesenteric blood-vessels of the animal with the lacteals in a state of activity, would be much more conspicuous than those of the one with the lacteals in a state of rest. In a trial instituted in the presence of Dr Traill, the difference in the diameter of the gastric vessels of the animal injected with its stomach full, and that with its stomach empty, was very apparent; and to me it appeared much greater than I had imagined it could have been.

PART II.

CRITICAL ANALYSIS.

I.

An Exposition of the Natural System of the Nerves of the Human Body, with a republication of the Papers delivered to the Royal Society on the Subject of the Nerves. By Charles Bell. London, &c. 1824.

In our Number for January 1824, we commenced an analysis of the researches, which have been for some time carried on chiefly throughout the Continent into the properties and functions of the nervous system; and we had given a combined view of the results obtained by several French and Italian physiologists regarding various parts of the brain and spinal marrow. We then announced, that some interesting observations had likewise been made on the uses and properties of individual nerves; but delayed taking notice of them, as we conceived it probable that new discoveries were on the eve of being made, which would materially influence the results previously obtained, and that consequently we might be enabled, by a little delay, to present our readers with a more precise and stable body of doctrine respecting the whole physiology of the nervous system.

It must be confessed that some of the views discussed in the paper alluded to,—namely those relative to the functions of the cerebellum and brain proper,—were always regarded in this country with distrust. This distrust has not been by any means diminished by the inquiries which have been made since; for not only have the results of various observers varied considerably, but likewise strong doubts have even been thrown upon the legitimacy of the whole method of investigation. As the facts which have been brought forward, however, are not strong enough to justify the impartial critic in forming even a probable opinion upon the questions at issue, we shall leave them entirely for the present, and proceed to redeem our pledge, by giving some account of the discoveries recently made in the physiology of the spinal chord and individual nerves.

If doubt and uncertainty have been thrown over the doctrines advanced by Flourens and others concerning the functions of the brain and cerebellum, the case has been very different with the opinions advanced on the subject of the spine and nerves. It has been clearly established, that different properties reside in different parts of the spinal marrow; and the labours of all experimenters agree in determining that different nervous filaments are appropriated to the different purposes of sensation and of motion.

By far the most important result of the competition that has prevailed in this department of medical science, is the publication by Mr Charles Bell of a connected account of his views regarding the Natural System of the nerves. A considerable part of his observations and discoveries had been read from time to time before the Royal Society of London betwixt the years 1821 and 1823, and were subsequently published in the Philosophical Transactions; but they had not attracted the attention which they deserved, chiefly, perhaps, as Mr Bell himself imagines, because these detached treatises "did not convey an idea of the system, or of the simplicity and order which are introduced by it into the demonstration of the nerves;" and partly, too, it is suspected, because some of his professional brethren, whose opinion is of weight in such matters, had (for reasons we need not inquire into) received his statements with less of that attention and disinterestedness to which their importance and the high reputation of their author unquestionably entitled them. For these reasons, although Mr Bell appears to complain of his being compelled to publish a connected view of his inquiries prematurely, yet we have no doubt he has acted wisely; and it may be added, that the step he has taken was still more imperiously called for, in consequence of the claims which his friends raised on his behalf to discoveries appropriated by other physiologists. On this particular part of the subject, we must be excused for adding, the public had reason to expect fuller details than he has deemed it fit to give personally.

We shall now proceed to analyze the contents of his work, adding occasionally such illustrations and strictures as may be derived from the contemporary and subsequent experiments of others, and from the controversy which the claim alluded to has called forth. It contains two distinct investigations. In the one he endeavours to show that each nervous thread is capable of conveying but one impression, and consequently that different twigs are destined for conveying the impressions which gave rise to sensation and those which excite muscular contraction: In the other, that a particular set of nerves, agreeing in their origin, structure, and mode of distribution, are appropriated to the purpose of regulating the various actions connected directly or indirectly with respiration. The former of these doctrines, which we noticed briefly on a previous occasion, appears to be now fairly established upon the fullest anatomical and experimental evidence; and the latter, to say the least of it, is rendered extremely probable by the exposition and arguments of Mr Bell.

It is long since upon theoretical grounds different nerves were supposed to be destined for the functions of sensation and of motion. This appears to have been the opinion of Galen. But for the most part, and especially in modern times, anatomists and physiologists espoused the opposite doctrine; and it was believed, that, with a few exceptions, (and even these were but vaguely ascertained), all the nerves proceeding from the brain and spine conveyed both kinds of impressions. The method by which Mr Bell was led to doubt the accuracy of this opinion, as we gather partly from himself, and still more distinctly from several essays published on the subject by his relation and pupil Mr Shaw, was the following. Reflecting that the third and ninth cerebral nerves were generally believed to be purely *motors*, and that these nerves arise from a part of the brain which is conceived to be continuous with the anterior column of the spinal marrow, he considered it possible, that this part of the spinal marrow and the corresponding part of the brain might differ in function from the posterior part, and might be intended specially for exciting voluntary motion. The composition of the spinal nerves, which all consist of two sets of roots, one from the posterior, and the other from the anterior column, enabled him to determine the point experimentally; and accordingly he found, that, on laying bare the roots of these nerves, he could cut across the posterior fasciculus without convulsing the muscles of the back, but that, on touching the anterior fasciculus with the point of the knife, the muscles of the back were immediately convulsed. * These facts were announced, so early as 1809, in a pamphlet which was printed for distribution among his friends, but not published. But, although according to his own account and that of his pupil Mr Shaw, they were never lost sight of from that time till the publication of his first paper on the subject in the Philosophical Transactions for 1821, we must be permitted to doubt, whether they were regarded by him, till very lately, with that intensity of interest which they could not have failed to attract, had their full scope and bearing been earlier apparent. This will appear, we think, on simply comparing the anatomical views he now takes of the structure and distribution of the nerves with the able description of them contained in the last edition of his work of anatomy published in conjunction with his brother in 1816. However this may be, we find him again assiduously employed on the subject early in 1821, at a period considerably antecedent to the date of M. Magendie's discoveries on the same topic. For Mr Shaw proves by extracts from his notes, that at that time Mr Bell and he had not only satisfied themselves of the accuracy of the experiments made in 1809, but had likewise so far extended their previous results to the spinal marrow itself, as to ascertain that the muscles were not convulsed when its posterior column was irritated in the upper part of the back.[*]

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* Shaw in Med. and Phys. Journ. 48, 458.

The train of Mr Bell's researches now led him away, as we shall presently find, to the second subject of inquiry, namely the appropriation of a particular set of nerves to all the actions directly or indirectly connected with respiration. But before accompanying him through these curious researches, we must leave him for an instant, to ascertain what light may be thrown upon his earlier discoveries by the more recent experiments of other physiologists.

It is to be clearly understood, that so early as 1809 Mr Bell was aware that the anterior roots only of the spinal nerves were distined for exciting voluntary motion. Of course he must have inferred, that the posterior roots were destined for sensation. But this was not determined experimentally, either at that time or in his subsequent researches with Mr Shaw in 1821; because, from motives of humanity, as it appears, he made his experiments not during life, but during the short period after death, when the nerves preserve their impressibility and the muscles their contractility, but when no proof can be procured with regard to the seat or conveyance of sensation.

The experiments of Magendie, of which we gave a succinct account in our Number for January 1824, were not published till August 1822; and as they were published only in the form of a very short notice, it is to be presumed that they had been

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[†] Shaw in Med. and Phys. Journ. 346-7.

but recently performed. The claims of Mr Bell to the dignity of original discoverer are therefore placed beyond all doubt, so far as regards Magendie. At the same time it can hardly be denied that Magendie is entitled to rank as second discoverer; for it is most improbable, that he had ever seen the Essay which Mr Bell circulated among his private friends thirteen years before, and he at once admitted that gentleman's claim to priority of discovery, when the Essay alluded to was sent him by Mr Shaw. How far he may have been led to make his experiments by the contents of Mr Bell's late papers in the Philosophical Transactions, in which, however, no distinct allusion is ever made to the researches of 1809, we presume not to judge. Magendie himself alleges he was led to make them, merely because he had long had them in contemplation. Another claimant to this discovery has likewise appeared in the person of Mr Herbert Mayo, lecturer on anatomy in London. This gentleman mentions in his Anatomical and Physiological Commentaries, that, being aware of the composition of the fifth cerebral nerve at its origin being the same as that of the spinal nerves, and having ascertained that its twigs which pass through the ganglion of Gasserius are destined for sensation only, while those which do not are intended for exciting muscular motion, he was led to conjecture that the posterior and anterior roots of the spinal nerves were similarly circumstanced; and he adds that he was engaged in experiments to determine the fact, when the publication of Magendie's rendered his unnecessary.* As in the latter part of this statement he lays claim only to the merit of intending to be a discoverer, it may be passed without further notice. It will be seen by and by, how far he is entitled to claim the discovery of the fact which led to his conjecture regarding the spinal nerves, namely the appropriation of different fasciculi in the fifth cerebral nerve to the purposes of sensation and motion.

It was mentioned in our former paper (vol. xxi. p. 156), that the opinion of Mr Bell and M. Magendie regarding the distinct functions of the roots of the spinal nerves has received ample confirmation from the subsequent experiments of M. Fodéra, which our readers will find detailed in the 3d volume of Magendie's Journal.

Bellingeri of Turin is, so far as we know, the only other physiologist of note who has occupied himself with these researches. His inquiries are partly recorded in a distinct work, published in 1823, † and partly announced in a short notice in the Annali Universali de Medicina for March and July 1824. The results at which he has arrived were founded in the first instance on a minute anatomical examination of the spine and spinal nerves, and on analogies in the physiological properties of other nerves, and are represented in the notices alluded to as having since been established by experiment. They differ somewhat from those obtained by Bell and Magendie. In common with some other anatomists, he represents the spinal marrow as consisting of six columns, two anterior, two lateral, and two posterior. The groove which divides the anterior columns from each other is for the most part distinct along the whole course of the cord; and that which separates the posterior columns is still more so; but the posterior lateral, and anterior lateral grooves, especially the latter, are here and there interrupted. There likewise exists in the internal structure of the spinal cord a good reason for the foregoing division: The cortical substance, as shown by a transverse section, is disposed in the centre after this form)—(; the points of the figure being directed forwards and backwards, and approaching or even sometimes actually meeting the four lateral grooves, while the middle anterior groove approaches, and the posterior one meets the transverse bar. The spinal nerves take their origin by two distinct roots, of which the posterior alone form ganglions. Each root, however, consists of three sets of filaments: The anterior are derived partly from the anterior column, partly from the lateral column, and partly from the bottom of the anterior-lateral grooves; and the posterior roots are derived from the posterior columns, lateral columns, and posterior-lateral grooves. The filaments which arise from the posterior-lateral grooves, penetrate to the posterior angles formed by the cineritious substance of the cord, and arise from it; but a corresponding junction of the filaments of the anterior lateral grooves with the anterior angles of the cineritious substance can be discerned only in the lumbar and sacral regions. Such being his observations on the anatomical structure of the spine and the origin of its nerves, he endeavours next to show upon analogical grounds, that the filaments which arise from the lateral columns, and which are common to both the posterior and anterior nervous roots, are destined to superintend the organic or instinctive functions; that the filaments arising from

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* Anat, and Physiol. Comment. II. 10.

† De Medulla Spinali, nervisque ex eo prodeuntibus Annotationes Anatomico-

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physiologicæ, auctore, C. F. Bellingeri, &c. This work has also obtained a place in the 28th volume of the Turin Transactions; and an interesting analysis of its contents may be seen in the 29th and 30th volumes of the Annali Universali di Medicina.

the posterior-lateral grooves convey sensible impressions; that those which take their origin in the posterior and anterior columns convey the impressions that excite muscular contraction; and that of these latter the posterior and anterior are antagonists to one another, the filaments from the posterior columns of the spine governing the muscles of extension, and the filaments from the anterior columns governing the muscles of flexion. With regard to those filaments of the anterior roots which arise from the anterior lateral grooves, as their precise origin is uncertain, he forbears inquiring at present into their destination.

We shall not detain our readers with the analogical arguments which the ingenious author has advanced in support of his opinions, but shall proceed to state the results of his subsequent experiments. According to a brief notice drawn up by Omodei the editor of the Italian *Annals of Medicine*, and represented to be an advertisement of the contents of a paper read before the Royal Academy of Turin in February and June 1824, Bellingeri has found experimentally, that the posterior roots of the lumbar and sacral nerves determine the extension of the hind legs; that the anterior roots determine their flexion; and that the posterior roots alone preside over the sense of touch. As no account has yet been published of the experiments on which these statements are founded, we abstain from offering an opinion on their validity. The physiological analogies which led the author to them are, to say the least, of a very vague description. It will be perceived, that he agrees with Mr Bell, M. Magendie and M. Fodéra in considering the posterior roots alone as intended for conveying sensible impressions, while he differs from them in assigning to these roots, as well as to the anterior ones, the power of directing muscular contraction.

The four physiologists just named have likewise extended their researches to the spinal cord itself; but they differ much from one another as to the properties of its several columns. Mr Bell's experiments on this subject are few and not quite conclusive. In March 1821, according to the notes of Mr Shaw, it was found, that in an animal recently killed, irritating the posterior columns did not cause muscular contraction. In 1823 Magendie found that no pain was caused by irritating the surface of the spinal cord between the anterior roots of the nerves of each side, but that the muscles supplied by them were thrown into violent contraction; and, on the contrary, that acute pain was caused by irritating the posterior columns of the spine between the posterior roots of the nerves. He does not say whether or not in the latter case he found any contraction to be caused in the muscles supplied by these nerves; but it is a curious circumstance, that muscular contraction does take place in parts which receive their nerves from the cord lower down than the seat of the irritation. The results obtained by Fodéra and Bellingeri do not coincide altogether with those of Magendie. We need not mention again the experiments of Fodéra.[*] We see no reason to alter the opinion formerly expressed regarding them, namely, that they are too incomplete to support any solid conclusion. Nay, the inquiries of Bellingeri render this opinion doubly strong; for Fodéra's observations were founded on the effects produced by cutting across the several columns of the spine,—an operation which cannot fail to cause very complicated results, if the views of the Italian physiologist are correct.

Bellingeri states it to be the result of his experiments, that the sense of touch resides only in the cortical substance of the spine, and that the medullary matter alone is concerned in the production of muscular motion. In conformity, too, with what he is represented to have observed with regard to the posterior and anterior roots of the spinal nerves, he found that "the posterior columns of the spine produce only the movements of extension, while the anterior columns determine only the movements of flexion in the extremities."[†] He adds, that the posterior columns relax the sphincter of the urinary bladder, and constrict that of the rectum; while on the other hand, the anterior columns relax the sphincter ani and constrict the sphincter of the bladder. As to the lateral columns, they do not serve for sensation, but preside over the organic functions, and influence the force of voluntary motion. Thus, then, he agrees with Magendie in assigning only to the posterior roots of the nerves the power of conveying sensible impressions, but differs from him and all other experimenters, in distributing over the whole surface of the spinal cord the power of exciting motion under stimuli, and in assigning the power of causing sensation to no part of that surface, but only to the cortical matter in the very substance of the cord. These statements are sufficient to prove that no sound conclusion can be drawn from the experiments of Fodéra, made by dividing halves, thirds, or fourth parts of the cord. They will likewise serve to show, that in attempting to determine,—even by pricking, and tearing, and pinching the cord,—what parts of it preside over sensation, and what other parts over muscular motion, greater precaution must be observed than Magendie himself was aware of. And this consideration will go far to account for many of the discrepancies which, it must be evident to every reader, prevail between the observations of the French and of the Italian physiologist; nay, even for that most important of all, the very different seat assigned by them to sensation. It must be remarked, however, that Magendie's experiments are essentially at variance with those of Bellingeri regarding the seat of sensation being in the cortical matter; for the former found that a stilet might be thrust along the centre of the cord, where the cortical matter abounds, without causing either sensation or motion. It is impossible even to conjecture a cause for that discrepancy in their statements.

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[*] xxi. 156.

[†] Annali Univ. de Med. 31, 163.

Here, however, we must again leave this interesting but complicated subject. It is clear that we cannot arrive at a decided conclusion on any point, except that the conveyance of sensible impressions depends on the posterior roots of the spinal nerves. To this result we are led by the researches of every physiologist who has hitherto entered the field of investigation. It may be added, that of late several interesting cases have been published of spinal disease in the human subject,—which strengthen still farther by their phenomena the result now mentioned, and which, in our former state of knowledge respecting the physiology of the nervous system, would have been quite inexplicable. Of these, we may notice, for the sake of illustration, three very striking examples. In the Annales de Chimie et de Physique, Magendie notices the case of a man who had lost the use of his arms, but retained completely their sensibility, and in whose body it was found, that the posterior roots of the nerves, which form the axillary plexus, were entire, while their anterior roots were reduced by the loss of medullary matter almost to their mere membranous covering. (vol. xxiii. 432.) In the same paper, another case to the same effect is mentioned on the authority of Royer Collard. This person had lost the power of motion over the whole body, but preserved sensibility; and on examining the body after death Royer Collard found evident disease in the whole anterior part of the spinal cord. (435.) The last we shall notice is detailed with great minuteness and apparent fidelity by Dr Koreff, in the fourth volume of Magendie's Journal. In this instance there existed complete palsy of both legs, and imperfect palsy of both arms, while the sensibility was everywhere preternaturally acute. "The medullary matter of the cord was singularly contracted in volume, the anterior part of its upper half was reddish-coloured, and, as it were, macerated, the roots of the nerves inserted there were so wasted as to be hardly discernible, but the posterior roots were unnaturally large."

While we notice these cases, it must at the same time be acknowledged, that others have occurred which do not appear to be quite so reconcilable with the doctrines we have laid down. We shall forbear mentioning them, however, as they are not very satisfactory, and as it is very probable that they will ere long receive a sufficient explanation, when physiologists have made up their minds on the points which remain undecided concerning the functions of the different parts of the spine.

Having now given an account of the development which Mr Bell's views on this subject have received from the contemporary and subsequent experiments of other physiologists, and endeavoured to settle how much of them may be adopted as now resting on a substantial foundation, we should return to where we left him, and follow out the train of his researches regarding the nerves which govern the various actions connected with the function of breathing. But as it is particularly desirable to put this department of his inquiries in the clearest possible light, we shall depart from the order of discovery, and attempt to arrive at his ultimate doctrines by a route which subsequent observation and experiment has rendered the most secure and the most intelligible. We shall proceed, then, in the first instance to apply to the nerves of the brain the discoveries made regarding the functions of the several roots of the spinal nerves.

The first circumstance which it is proper to attend to in drawing a comparison between the spinal and cerebral nerves, is their relative origin. Here it is necessary to call to remembrance at the outset, that, according to the doctrines of modern anatomy, the posterior columns of the spinal cord are in communication, through the Pons varolii, with the crura of the cerebellum, and the anterior columns are related in like manner to the crura of the brain proper.—Anatomists are not quite agreed as to the precise origin of all the cerebral nerves. Some, however, have been accurately determined; and in their case a very curious relation will be found betwixt their origin and the functions over which they preside.

As we ascend from the spine towards the head, we first observe the Spinal Accessory nerve. The best account of its origin is given by Bellingeri, who states, as the result of a very careful examination, that it arises by numerous roots from the lateral columns only of the upper part of the spinal column,—that it does not derive any twigs from the *posterior* roots of the cervical nerves,—that it is often apparently united at its origin with these roots, but that the apparent union is seldom any thing more than simple approximation,—and that, when the posterior roots of the first cervical nerve do send a twig to the accessory nerve, this twig soon leaves it again, and really forms after all a part of the cervical nerve. The result of this description is, that the spinal accessory has no roots corresponding with those, which, in the case of the spinal nerves, convey sensible impressions. Bellingeri's account agrees very nearly with that of Mr Bell, p. 201. The next nerve that presents itself, as we proceed upwards, is the Ninth, which rises from the lateral or anterior part of the medulla oblongata, between the corpora olivaria, and pyramidalia; it has not therefore an origin corresponding with the posterior roots of the spinal nerves. The Eighth nerve rises, according to Mr Bell, from the lateral part of the medulla oblongata; but sometimes, according to Soemmering, both the glosso-pharyngeal branch and the par vagum derive some of their roots from the crura of the cerebellum, or from the fourth ventricle. The portio dura of the Seventh nerve rises from the lateral part of the medulla oblongata, the portio mollis from the fourth ventricle. The Sixth nerve rises from the corpora pyramidulia, and has no posterior roots. The Fifth nerve rises chiefly from the crura of the cerebellum, where they unite with the pons varolii; but a portion of it plunges between the *funes* of the pons, and actually arises from the crura of the brain proper, (Bell 38). Hence this nerve has a double origin, like the nerves of the spinal cord. The Fourth nerve arises where the crura cerebri are connected with the corpora quadrigemina, (Bell 38); and the Third arises from the crura of the brain proper. Consequently both of these nerves arise only from the prolongations of the anterior columns of the spinal cord. The olfactory and optic nerves take their origin from the brain, at a distance from its connexions with the cord; and, together with the portio mollis of the seventh, bear scarcely any relation to the other nerves either in origin or properties: They neither convey those impressions which excite muscular motion, nor do they possess the sensibility of the other nerves of sense. * They can convey only the single impressions peculiar to the organ to which they are distributed. It appears, then, from this summary, that of the cerebral nerves, two only resemble those of the spine, in having a double origin corresponding with the anterior and posterior roots of the latter, namely, the fifth pair and seventh pair; that the third, fourth, sixth, ninth, and spinal accessory have but one root, corresponding with the anterior roots of the spinal nerve; and that some ambiguity hangs over the origin of the eighth nerve, while on the whole it resembles the spinal accessory.

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* Journ. de Physiol. iv. 170, 312, 314.

The second circumstance to be attended to, in comparing the cerebral and spinal nerves, is their structure. The structure of the spinal nerves has been frequently examined with great care. The most recent description is that given by Bellingeri. He states, that the posterior roots are larger, but fewer in number than the anterior ones; that the former have a plexiform texture, while the filaments of the latter run parallel to each other, without uniting together; and that the ganglions of these nerves are formed on the posterior roots, before they unite with the anterior ones, and do not include the latter. When we apply this description to the nerves of the brain, we find a striking relation to subsist betwixt their origin and their structure. Thus, in the case of the fifth cerebral nerve, which has a double origin, we remark that the roots from the crura of the cerebellum are larger than those from the crura cerebri, and that the former alone pass through the Ganglion Gasserii. In the next place, we may remark of the seventh, that the portio dura, which arises similarly with the anterior roots of the spinal nerves, has a dense texture, and does not form a ganglion. As to the portio mollis, it scarcely bears any analogy to the spinal nerves, and is properly not a part of the seventh nerve, but a nerve by itself, endowed with peculiar properties. The spinal accessory, like the portio dura, has a dense texture, and seldom forms a ganglion; and if it does, it is only, according to Bellingeri, when the posterior root of the first cervical nerve is temporarily united with it. The ninth, sixth, fourth and third nerves, have no ganglion either, and with the exception of the fourth are dense in texture. The eighth resembles in its texture the spinal accessory, portio dura, and anterior roots of the spinal nerves; and according to Mr Bell, it has not a ganglion near its origin, (87). Mr Mayo, however, declares it has a ganglion (Anat. Comm. I. 114).

Thus it appears, that all the nerves which arise from parts corresponding with the anterior columns of the cord are similar to each other in texture, and agree with the anterior roots of the spinal roots in not forming ganglions,—that ganglions are only to be found on such nerves as have their origin also in parts of the brain corresponding with the posterior columns of the spine. The par vagum is the only exception to this rule; and it will not be an exception if it really derive its roots in part from the fourth ventricle and crura of the cerebellum.

Let us now pass, in the third place, to the most important of the points of comparison, and inquire what relation subsists between the nerves of the head and the several roots of the spinal nerves in their function. From the preceding account of their relation as to origin and structure it is natural to expect, that some will be nerves of motion only, while others will be nerves both of sensation and of motion; and it may even be predicted to which of the nerves these properties will belong.

The third, fourth, sixth, seventh, ninth and accessory nerves, ought to be nerves of motion only.

The third nerve is entirely distributed to muscles, (namely to the muscles of the eye) with the exception of a small twig which assists in forming the lenticular ganglion. Experiment has shown that it does not convey sensible impressions. Magendie found this to be the case.* Once or twice, indeed, he did observe slight traces of sensibility to pain; but we shall presently show, that this phenomenon is not inconsistent with the fact, that the nerve cannot properly convey the impressions connected with the sense of touch,—at least that most of the pure nerves of motion do possess the property of sensibility in a slight degree. Mr Mayo, it may be added, found that the division of this nerve does not impair the sensibility of the surface of the eye. †

The fourth nerve is distributed entirely to one muscle, the superior oblique of the eye, and the sixth entirely to the abductor of the eye; but the former interchanges communicating twigs with the sympathetic. The use of these communicating twigs is unknown. The experiments of Magendie leave no room to doubt, as reasoning had led Mr Bell to infer long before, that both of these nerves may be pinched, cut, or torn, without giving rise to sensation, that the only effect is convulsion of the muscles they severally supply.

The seventh nerve is likewise distributed entirely to muscles, namely, to the muscles of the whole face, the superficial muscles of the throat, and the muscles of the velum and back part of the palate. The experiments of Mr Bell and Mr Shaw, which have been amply confirmed by those of Mayo, Magendie and Foděra, have established that this nerve directs muscular motion, and has but very little, if any, power of conveying sensible impressions. All the parts supplied by it are instantly paralysed when it is divided, and thrown into convulsive contraction when it is pricked or torn; while at the same time their sensisibility is not in the slightest degree impaired by its division, nor does that operation cause any pain. On this last point, however, there is a considerable variety of statements. Mr Bell himself says, that no pain is expressed at all when the nerve or any of its branches is pricked or cut. On the other hand, Mr Shaw seems to admit that it has some degree of sensibility;[*] M. Mayo says, that in animals which are not patient of pain, as the dog or cat, its sensibility is distinct, though still much inferior to that of the true nerves of sensation.[†] Fodéra makes very nearly the same remarks;[‡] and Magendie observes, that, if we may judge from the struggles and cries of the animal, the nerve is *very* sensible.[§] Mr Shaw has pointed out one evident cause for its occasional sensibility, namely, the occasional addition to it of a branch of the fifth nerve, which, as will presently be seen, is the nerve of sensation for the face. But there can be little doubt, that the branches of the portio dura are also to a certain degree capable of conveying the sensation of pain, independently of that circumstance. Yet it must not on that account be inferred that it conveys sensible impressions, made on the parts to which it is ultimately distributed. For its division does not impair in the least the sensibility of these parts; and they do not retain any of their sensibility if the fifth nerve only be divided.[||]

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* Journal de Physiol. iv. 313.

† Anat. Comm. ii. 5.

The next cerebral nerve, which has been fairly proved to be a nerve of motion only, is the ninth or lingual nerve. It has long been thought to be merely the regulator of the muscular contractions of the tongue, as it is distributed only to the muscles of that organ, and various pathological cases have likewise supported that conclusion. But the fact has been definitively settled by positive experiment. Mr Mayo found, that when the ninth nerve was irritated in an animal recently killed, the tongue was convulsed, and that when the nerve of each side was divided, the tongue became motionless;[¶] and Fodéra has obtained precisely the same results.[**]

The spinal accessory nerve is the next we shall notice. This nerve too, by reason of its origin and structure should be a nerve of motion only. It is distributed chiefly to two muscles that raise the shoulders, the mastoid and the trapezius, and besides sends a branch to join the glosso-pharyngeal nerve. When it is irritated, the muscles are convulsed; when it is divided, they become incapable of joining in those involuntary actions by which the shoulders are raised for the purpose of forcible inspiration;

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[*] Med. Phys. Journ. 48 350.

[†] Anat. Comm. ii. 4.

[‡] Journ. de Physiol. iii. 206.

[§] Journ. de Physiol. ii. 67.

[||] There is an experiment indeed related by Fodéra, which does not accord with the statements above, (See Magendie's Journal, iii. 203); but it is liable to the objection, that twigs from nerves of sensation might have accompanied the twigs of the portio dura which he operated on.

[¶] Anat. Comm. i. 116, ii. 11.

[**] Journ. de Physiol. iii. 206.

but through means of the cervical nerves, they still remain susceptible of ordinary voluntary contraction, (Bell 213). Mr Mayo adds, that when irritated, considerable pain is expressed.[*] This may depend either on the property already noticed under the head of the portio dura, or on the stimulus having been applied beyond the point at which the accessory nerve is joined by twigs from the cervical nerves. It is evidently difficult to determine whether it can convey impressions made on the parts to which it is distributed. In the mean time, however, we are sure it cannot be a nerve of the proper sense of touch; for, as Bellingeri mentions, no part of it is sent to the skin.[†]

We have seen that there is some ambiguity with respect to the origin of the glosso-pharyngeal and the nerve of the par vagum. There is also some ambiguity as to their function. They are certainly distributed to muscles, namely, to the muscles at the back of the throat, root of the tongue, pharynx, larynx, œsophagus, diaphragm and cardiac end of the stomach; but they likewise send twigs to various branches of the sympathetic; and, according to some anatomists, the glosso-pharyngeal nerve supplies the papillæ at the root of the tongue.[‡] When they are irritated, the muscles they supply are convulsed, with the exception, according to Mayo, of the muscles of the stomach.[§] When they are divided, all the muscles which are visible become evidently paralyzed, unless they also receive nerves from other quarters; and so far as can be ascertained, sensation is nowhere destroyed. At the same time, at least one of these nerves, the par vagum, is acutely sensible. Many experimenters have remarked this. We have ourselves found it to be so sensible in the dog and cat, that, without delicate handling, it is difficult to divide it or separate it from its connexions on account of the struggles of the animal. It must be confessed, however, that in the region where it is usually operated on, it is by no means a simple nerve, but has received filaments from those in its neighbourhood. We may take the liberty of suggesting to Mr Bell in particular, the necessity of farther inquiry into the origin, distribution and function, both of this nerve and of the glosso-pharyngeal.

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[*] Anat. Comm. ii. 15.

[†] Annali Un. di Med. 31, 14.

[‡] On this subject it may be worth while to quote the following passages from the Tabulæ Neurologicæ of Scarpa. " Rami *linguales-laterales,*— - - - - Profecto certum est hos nervos in membrana vestiente marginem posteriorem linguæ, tenuissimorum penicillorum ad modum evanescere. - - - - - Rami *linguales profundi.* - - - - non possumus hac occasione sensubus nostris fidem denegare, cogimurque constituere Profundos ramos Glosso-pharyngei ad unum omnes in linguæ superficie desinere, nulla in itinere vel minima tradita fibrilla çarneæ linguæ texturæ, p. 19.

[§] II. 16.

The experiments which have been made upon its influence on digestion and the chemical processes carried on in the lungs, do not form a sound objection to the views taken by our author of its properties, as these experiments have not been made on the simple par vagum.

Of the nerves of the head, the only one remaining for consideration is the fifth. This nerve is the most interesting of all to the physiologist. It is only of late that a correct knowledge has been obtained of its functions, and for that knowledge we are indebted in the first instance entirely to Mr Bell and his friend Mr Shaw. Their experiments have been successfully repeated by others, and indeed an attempt has been made by one person even to assume the merit of original discoverer. That attempt, however, has failed; and ultimately the only consequence has been, that Mr Bell's researches have met with the strongest confirmation of which they were susceptible, the concurrence and envy of an eminent rival. We have seen, that in its origin and structure the fifth cerebral nerve has a close resemblance to the nerves of the spine; that it has a double root, one corresponding in situation with the posterior roots of the spinal nerves, and, like them, forming a ganglion; the other corresponding with their anterior roots, and like these roots, joining the rest beyond the ganglion. Mr Bell has explained this structure of the fifth nerve very beautifully; but as his critic, Mr Mayo, has properly remarked, it had been already laid down, at least in a great measure, by Soemmering. The distribution and function of the nerve correspond exactly with what we should infer them to be from its origin and structure. We are indebted to Mr Mayo for an interesting fact, which completes our knowledge of its distribution, namely, that the root from the crus cerebri, which passes by the ganglion of Gasserius, is entirely expended upon muscles, namely upon the pterygoids, masseter, temporal and circumflexus palati muscles, (ii. 9.) The greater and ganglionated root is distributed to all the parts of the face supplied by the portio dura of the seventh nerve, and likewise to the tongue and teeth, and globe of the eye. The experiments of Bell, Magendie, Mayo and Fodéra, all unite in showing that the branches of this root are nerves of sensation only, and that those of the root arising from the crus cerebri are nerves of motion. These experiments are so interesting and precise, that we shall give a short general abstract of them; and for the sake of perspicuity, we shall begin with those made last, namely, upon the trunk of the nerve itself.

Fodéra appears to have been the first who succeeded in dividing the whole fifth nerve before it emerges from the cranium. The subject of experiment was a rabbit; and he found, that the whole lower jaw, the tongue, palate and cheek, the inside and outside of the nose, and the eye-lids, were rendered insensible.[*] The experiment was repeated with greater care by Magendie, by dividing both nerves; and he found that the ball of the eye was likewise insensible, and that the muscles of the lower jaw were paralyzed. Mr Bell and Mr Shaw noticed that, when the origin of the fifth was irritated in an animal newly killed, the jaw snapped with great violence.[†] In farther illustration of these facts, Magendie relates, on the authority of Serres, a very interesting case in the human subject, in which there was complete disorganization of the whole ganglionated roots of the fifth nerve, and in which sensation was entirely lost over all the parts it supplies, while the motions of the lower jaw were unaffected.[‡]—A great number of experiments have been made on the several branches of this nerve. When the frontal division of its ophthalmic branch was divided in a man for tic douloureux, Mr Bell remarked, that sensation was lost in the parts it supplies, but not motion, (ii. 8.); which result is confirmed by an experiment of Mr Mayo on the ass.[§] When the proper ophthalmic branch is irritated in an animal newly killed, the muscles of the eye, as Mr Bell found, are not thrown into contraction, (p. 346.); but when it is divided (in common with other branches of the fifth) the eyeball and eyelids lose their sensibility so entirely, that, according to Magendie, they are not affected even by strong ammonia; while at the same time their motions continue uninjured.[‖] With regard to the superior maxillary branch, Mr Bell found, that, when it is pricked in a living animal, great pain is expressed, and when divided the upper lip loses its sensibility; but no muscular contraction is caused in the former instance, and no paralysis in the latter; while these effects on the muscles follow corresponding injuries done to the branches derived from the portio dura. (pp. 105, 106.) His experiments have been repeated with the same results by Magendie.[¶] Effects of the like kind take place, when the infra-orbitary divisions of this branch are divided in the ass (Bell, 117); but a curious fact mentioned by Mr Shaw, which we shall explain presently, was also remarked when both infra-orbitaries were divided; namely, that the upper lip was not used in picking up its food.[**] This might lead a careless observer to conclude that the infra-orbitary nerves are nerves of motion as well as of sensation. Corresponding effects follow the division of the inferior maxillary branch of the fifth. In one of Fodéra's experiments on the trunk of the nerve within the skull, he observed that the lower jaw and tongue alone retained their sensibility; and the cause was rendered evident by dissection after death; for the whole nerve had been divided except the inferior maxillary.[*] But the most striking phenomena are produced, when both the superior and inferior maxillary nerves are divided. Both lips become quite insensible; and at the same time the animal loses the power of gathering its food with them. It has been already remarked, that this power is taken from the upper lip by the division of the superior maxillary; the ass, when so circumstanced, gobbles up its oats with the lower lip, teeth and tongue.[†] When the inferior maxillaries are also divided, Mr Mayo remarked, that this operation is performed with the tongue and teeth only.[‡] It must not, however, be inferred on that account, that the fifth nerve presides over the motion of the lips. For both the last mentioned gentleman, and Messrs Bell and Shaw before him have remarked, that the movements of the lips are called forth on other occasions, and that their muscles are thrown into convulsions or paralyzed, not when the fifth, but when the seventh nerve is pricked or divided. The cause of their remaining motionless during the act of feeding appears to be, that, as they are insensible to the presence of the food, the corresponding muscular actions, which are instinctive or at least habitual, and depend on the previous stimulation of the nerves of sense, are not excited. It would be an interesting fact to determine whether or not time and practice would restore these muscular actions.—Many experiments have likewise been made on the minuter subdivisions of the three great branches of the fifth nerve. But as they all tend to the same conclusion as those already mentioned, we shall omit the greater part of them, and give only a short account of such as have been performed on the gustatory nerve, the most important division of the inferior maxillary branch. The gustatory nerves, according to Mr Mayo, are exceedingly sensible; but irritating them causes no motion in the tongue. When divided, the tongue retains its movements, but the sensibility of its surface is lost.[§] Fodéra repeated these experiments subsequently, and obtained the very same results, with the additional observation, that the sense of taste is altogether destroyed; for strong bitters caused neither disgust nor salivation.[*] Magendie likewise observed with Mayo, that the tongue loses sensibility and retains its movements, when the trunk of the fifth nerve is divided; but he differs from Fodéra in finding that the sense of taste is lost only on the tip, while it remains uninjured on the centre and base.[†] This discrepancy is easily explained, however, by reference to some recent ingenious researches of Chevreul into the physiology of the sense of taste, of which an abstract will be found in our Medical Intelligence of the present quarter.[‡] In the present place we need merely remark that, according to these researches, tastes in the ordinary acceptation of the term are of three kinds, impressions on the nerves of touch, impressions on the proper nerves of taste, and impressions on the nerves of smell. It is thence easy to conceive how the gustatory nerves may be divided, and yet certain tastes appear to remain.

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[*] Journ. de Physiol. III. 207. [†] Ibid. IV. 181, 176. [‡] Ibid. IV. 303.

[§] Anat. Comm. I. 110.

[‖] Journ. de Physiol. IV. 303. In a previous set of experiments, Magendie thought the motions of the eye were affected. Ibid. 177; but this afterwards proved to be an error.

[¶] Ann. de Chim. et de Phys. XXIII. 431.

[**] Med. Phys. Journ. XLVIII. 465.

[*] Journ. de Physiol. iii. 208. [†] Shaw, l. c. [‡] Anat. Comm. i. 110.

[§] Anat. Comm. ii. 10.

The amount of the preceding investigation into the properties of the fifth nerve, therefore, is, that as it bears a close analogy to the spinal nerves in origin and structure, so it resembles them in consisting of two divisions, one of which, corresponding to the anterior spinal roots, is a nerve of motion only, while the other, corresponding to the posterior spinal roots, is purely a nerve of sensation. In adducing evidence to this effect, we have been led, for the sake of perspicuity, to bring most prominently into view researches made subsequently to those of Mr Bell. It must be remembered, however, that he alone is entitled to the merit of the discovery; and we should add, that his own work perhaps contains evidence enough to prove the fact in question. In short subsequent physiologists have merely simplified or perfected his experiments, and added others obviously suggested by them.

Before quitting the subject of this nerve, we must add a few remarks on certain opinions advanced by Magendie, which assign to it an influence far more extensive than we have hitherto mentioned, and far more extensive and diversified than any other nerve possesses, or than any other physiologist had ever dreamed of ascribing to it. According to certain experiments detailed in his Journal for April 1824, with a great deal of apparent caution and would-be matter-of-fact simplicity, he has found that the olfactory nerve is not the nerve of smell, nor the optic nerve of sight, nor the acoustic nerve of hearing, but that the fifth nerve is necessary to the existence of all these senses; and although in a paper published in the next quarter, after announcing that his subsequent researches oblige him to change very few of his previous conclusions, he appears to admit so far the propriety of a change, as to grant the necessity of the optic nerve to the sense of sight, still the opinions which he retains as to the functions both of that nerve, and of the acoustics and olfactories, are calculated to overturn many of our most ancient and best established doctrines regarding them. The inferences he draws from his observations are so absurd, that if they came from a less eminent physiologist we should feel disposed to pass them over in silence. As the case stands, they deserve some notice; but it shall be very short. We shall say nothing of those parts of his inquiries which relate to the optic and acoustic nerves, and the relative branches of the fifth; the eagerness to publish, and dread of anticipation which are at present the ruling demons of most French physiologists, has led him to give them to the world in such an unformed state, that no impartial man can found an intelligible conclusion upon them. As to his observations on the olfactory nerves, and the nasal branches of the fifth, all we need say is, that the discoveries of Mr Bell respecting the function of the fifth nerve would lead us to infer that it is the organ of touch and its collateral sensations for the internal cavity of the nose,—that these sensations must be sometimes confounded with smells, properly so called, as Magendie has evidently done, notwithstanding his apparent care to avoid the error,—that some other sensations, compounded of smell and touch, will certainly be modified by the destruction of the fifth nerves on the one hand, and of the olfactory nerves on the other,—but that Magendie has not advanced a single fact to show, that the proper sense of smell is retained when the olfactories are alone divided, or lost when they alone are left entire.

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[*] Journ. de Physiol. III, 206. [†] Ibid. IV. 181. [‡] Ibid. IV. 127.

In stating the physiological discoveries of Mr Bell, and his rivals and coadjutors, we have hitherto confined our attention almost exclusively to matters of fact. We have now to conclude this analysis with some account of the leading object of Mr Bell's work, namely, his theory regarding the combined acts of respiration, and the arrangement of nerves by which these combinations are effected.

According to the theory in question, the various acts of respiration, including both those in which respiration is actually called forth, and those immediately connected with it, are governed by a set of nerves distinct from those of true voluntary motion, and resembling each other in origin, distribution and properties. The actions which, under Mr Bell's views, may be accounted respiratory actions, are breathing in its various conditions from natural to laborious breathing, sighing, crying, laughing, smelling, sneezing, coughing, yawning, speaking, singing, swallowing, vomiting, and the various muscular actions produced by the expression of the passions. These are all very complicated actions. Even in natural breathing, the diaphragm, the intercostal muscles, and some others which have not yet been very satisfactorily determined by physiologists operate simultaneously; when the breathing becomes laborious, the greater muscles of the ribs, those of the shoulders and throat, and likewise those of the mouth and nostrils, are made to participate; and in some of the collateral actions, such as sneezing for example, the muscles of the eyelids, eyebrows, and whole face, as well as those of the abdomen, are powerfully excited. In order to unite all these parts in harmony together, Mr Bell conceives that they are supplied with certain nerves having a common origin, and differing in structure, mode of distribution, and properties, from those which govern the voluntary contraction of the muscles. The place from which they arise is the lateral column of the spinal cord, while the nerves of sensation and voluntary motion rise from the posterior and anterior columns, or from both together. They are dense in texture, and are composed of very minute filaments. Their distribution is only to such muscles as are concerned in the subsidiary acts of respiration; they often run longitudinally to the body, which is never the case with the other nerves. The chief peculiarities in their properties are, that they are very little, if at all, susceptible of those impressions which excite sensation, but are peculiarly susceptible of those which excite muscular contraction; and that they retain their impressibility longer after death than other nerves. On account of their supposed purpose, they are called by our author Respiratory Nerves, in contradistinction to the Motor and Sensitive Nerves. They are the Fourth nerve of the Head, the Portio Dura of the seventh, the Par Vagum, the Glosso-pharyngeal, the Spinal accessory, and the Phrenic nerve; to which Mr Bell adds, though not upon what he conceives to be perfect evidence, the External Thoracic, which proceeds from the fourth and fifth cervicals, and is distributed to the *serratus magnus* muscle. The names assigned to these nerves, according to the new system, are the following—the Respiratory nerve of the Eye (fourth of the head), the Respiratory nerve of the face (Portio dura), the Par Vagum (whose name has been unaccountably retained), the Superior Respiratory nerve of the trunk (Spinal Accessory), the Internal Respiratory (Phrenic), and the Inferior Respiratory nerve of the trunk, (nerve of the Serratus).

The proofs of the validity of these views of the natural system of the nerves are drawn, first, from anatomical origin, structure and distribution; secondly, from observation of the effects of injuries produced in man by disease, and in brutes by direct experiment; and thirdly, from comparative anatomy, that is from the presence and absence of respiratory nerves or branches of them, according to the presence or absence of respiratory organs, whether essential or subsidiary.

1. Our examination of his theory has already embraced a succinct account of the proofs derived from origin and anatomical structure and distribution. The similarity of their origin is certainly striking. Exclusive of the phrenic nerve and that of the serratus magnus, they arise by simple roots, in a line from the lateral columns of the spinal cord. The origin of the two remaining nerves has not been satisfactorily determined; but we may infer from the researches of Bellingeri, that they too may possibly arise from the same quarter, although their roots are apparently connected with the whole substance of the cervicals. Mr Bell even asks the question, whether respiratory roots may not exist for the whole spinal nerves; and here too his conjecture may be borne out by the dissections of Bellingeri. The distribution of these nerves only to such muscles as are concerned in the essential and subsidiary acts of respiration, is another very striking feature in their anatomical character. But it is not altogether without exception, as we have formerly noticed under the head of the eighth nerve.

2. The proofs derived from the observation of the effects of disease and the phenomena of experiments, are of course by far the most convincing, if they are deduced with proper caution from well ascertained facts. We shall therefore endeavour to place this part of the subject in as strong a point of view as possible.

The first of the respiratory nerves which Mr Bell made the subject of minute investigation, was that of the face, or the portio dura of the seventh cerebral nerve. He found by frequent experiment, that, when this nerve was divided in the lower animals on one side, they lost completely all power of expression on that side; the dog could not snarl, the monkey could not make wry faces, and the ass could not curl the nose with the expression of sneezing, when the nostrils were irritated. If the breathing was rendered laborious by any means, the movements of the nostrils and lips which accompany that state, were entirely confined to the side of which the nerve was sound; and if the animal was bled to death, those convulsive motions of the face which invariably precede dissolution and accompany the convulsive contractions of the regular muscles of respiration, were likewise confined to the sound side. When the nerve was divided on both sides, all these movements, both of expression and of convulsive breathing were totally lost; and farther, the lips ceased to be employed in gathering food, and the orbicularis palpebrarum no longer closed the eyelids. This last fact is one of much importance in operative surgery; for, as the loss of contractility in the orbicularis palpebrarum causes great liability to inflammation of the eye, it is necessary to avoid wounding, in operations on the face, the twigs of the portio dura which supply that muscle.

The facts established by Mr Bell regarding the very different functions fulfilled by the portio dura and fifth nerve, will serve to explain many singular symptoms of partial palsy of the face. They show how sometimes the sensation alone, and sometimes only the movements of one side of the face may be lost; how certain trifling actions of the muscles of the face may be lost, while the rest continue; how the sensation of a small part of the face may be lost, while that of the rest and all the muscular actions continue; how the muscular motions of the face may be lost, and the action of mastication remain unimpaired; how the face may be completely palsied on one side, without indicating the slightest danger to life. Accordingly, Mr Bell has brought forward several very interesting cases, which admit of most singular elucidation from his discoveries, and which, at the same time, confirm in the clearest manner their accuracy. We regret our limits will not permit us to transfer any of these to our pages; and must rest satisfied with rousing the reader's curiosity by the following short notice.

"A man had the trunk of the respiratory nerve of the face injured by a suppuration which took place anterior to the ear, and through which the nerve passed in its course to the face. It was observed, that in smiling and laughing, his mouth was drawn in a very remarkable manner to the opposite side. The attempt to whistle was attended with a ludicrous distortion of the lips; when he took snuff and sneezed, the side where the suppuration had affected the nerve remained placid, while the opposite side exhibited the usual distortion." p. 114.

The next nerve which Mr Bell seems to have succeeded in associating with the respiratory actions, upon fair experimental evidence, is the spinal accessory, or his superior respiratory nerve of the trunk. This nerve supplies the sterno-cleido-mastoid and the trapezius muscle, whose action is to raise the shoulders and chest, and to bring together and fix the scapulæ.

These actions are called forth involuntarily to assist in laborious respiration, but they may likewise be excited voluntarily for other purposes. On these accounts, according to the views of Mr Bell, the two muscles have two different kinds of nerves, the ordinary nerves of voluntary motion, and a respiratory nerve. Accordingly, if the spinal accessory be exposed on each side of an ass,—in which it is distributed to the trapezius and to two muscles similar in function, and analogous though not quite similar in position to the mastoid in man,—and laborious breathing be induced, the muscles it supplies act powerfully with the other muscles of respiration; but if it be divided, they cease to act on that occasion, though they still act as voluntary muscles, (p. 214.) The counter part of this experiment occurred in the case of a person affected with perfect hemiplegia. He could not raise the shoulder by an exertion of the will; when told to do so, he succeeded only by bending the spine to the opposite side. But when he was told to inspire forcibly, " both shoulders were elevated at the same time that both the nostrils were in motion, " (212.) This fact is a very striking one, because, the palsy affecting the face, the cause must have been in the head; yet the injury thence communicated to the spinal cord must have affected its anterior and not its lateral columns.

It is needless to enter into any detail regarding the phrenic nerve, which every one knows to be a nerve of respiration only.

Upon the external thoracic, or inferior respiratory nerve of the trunk, Mr Bell has not made any experiments; and he has collected but one observation. In a case of fracture of the sixth and seventh vertebræ of the neck, in which the whole body under the seat of the injury was powerless, Mr Bell remarked, that the serratus magnus was called into action with every extraordinary effort of the breathing, but not by any efforts at voluntary motion, (p. 228.)

These four, with the intercostal nerves, constitute the nerves of the simple respiratory actions. Their respective influence is very distinctly shown by first dividing the phrenic, then the spinal cord at the bottom of the neck, and finally the medulla oblongata. In the first instance, the movements of the chest become more expanded, but without the subsidiary muscles being called into action, unless when the breathing is rendered forcible. Next, the chest remains immoveable, but the muscles of the face and neck are violently convulsed in ineffectual endeavours to maintain respiration. Lastly, the nostrils and lips alone exhibit these phenomena.

The glosso-pharyngeal and par vagum superintend both the simple and the collateral respiratory actions. Of the latter, the most obvious are, speaking, singing, swallowing, vomiting, coughing, and the like. In all these actions, the office of the eighth nerve should be to unite the muscles of the throat, windpipe, and gullet with the other muscles of respiration; and that such is really their office has been amply proved, at least with regard to some of their branches, by frequent experiments on the lower animals. We have seen, however, that some ambiguity prevails, as to the origin and properties of the glosso-pharyngeal and par vagum branches of the eighth nerve,—that not only their origin, but likewise their distribution, and the effects of irritants applied to them, would tend to confer upon them other functions. It may now be added, that our author has not advanced any evidence to prove that they are not, in part at least, the nerves of voluntary motion for the muscles of the throat, larynx, and gullet. In many respects, therefore, the physiology of the eighth nerve is incomplete, and requires farther elucidation, before it can be fairly arranged with the other respiratory nerves.

Perhaps the least satisfactory, and certainly the most complicated department of Mr Bell's inquiries, is that relative to the share of the fourth nerve in the subsidiary actions of respiration. We regret that our limits compel us to be content with a very short account of it; and in particular, that we must omit altogether several novel and ingenious speculations, which he has intermingled upon various extraneous subjects connected with the physiology of the eye. The superior oblique muscle, on which the fourth nerve is entirely expended, rolls the eye downwards and outwards; the inferior oblique rolls it upwards and inwards. But the recti muscles are of themselves sufficient for executing these movements; and our author therefore concludes, that while the recti muscles are intended for the voluntary movements, the oblique muscles are intended for involuntary motion. This involuntary motion is a turning of the eye upwards and inwards; which takes place, in the first place, during sleep; secondly, when the eyelids close, to protect the eye from external violence, and from the internal impulse given to it during the act of sneezing; thirdly, in fainting, in excessive debility from various causes, and during the convulsive respiration which precedes death; and, fourthly, in the expression of various passions of the mind; in all of which actions it is more or less directly associated with the essential and subsidiary muscles of respiration. The principal difficulties the author has to encounter in thus arranging the fourth with the respiratory nerves are, that it arises from a situation in the brain, to which the lateral, or respiratory columns of the spine, according to his own account, do not extend,—that he makes the inferior oblique an involuntary muscle, as well as the superior oblique, although it is supplied with a nerve belonging not to the respiratory, but to the motor system,—and that he is obliged to assume that the fourth nerve does not make its muscle contract simultaneously with the other respiratory muscles, but makes it fall into a state of relaxation, while all the rest contract. The last difficulty he has endeavoured to obviate by analogical facts, but he has barely touched upon the second, and has not alluded to the first at all.

3. The last source from which Mr Bell has drawn evidence of the validity of his new system of the nerves is comparative anatomy. This science furnishes an ample field of research, from which he has derived many interesting facts and analogies. The essence of this part of his inquiry is, that whenever certain muscles, which in some animals form a part of the respiratory muscular system, are wanting in others, or, though present, are not called into action during any of the respiratory acts, then the corresponding nerve, derived in the former animals from one or other of the respiratory nerves, is wanting in the latter.

Thus, the par vagum does not exist, " but where there are heart and lungs to associate with a muscular apparatus and respiration." Thus, too, the proportion of the portio dura to the fifth nerve is greater in man than in other animals, because its office as a respiratory nerve is comparatively more extensive; and, among different animals, its proportion and intricacy of distribution bear a steady ratio to the extent of its respiratory office in each. It is greater and more minutely subdivided in the monkey than in any other animal; in the lion, dog, and cat, than in the horse, ass, and cow; in the horse, than in the sheep or deer; and in the camel, than in the horse. Accordingly, in the sheep, deer, and cow, we remark that the lips, nostrils, and cheeks are associated only with the simplest respiratory acts, while in the horse and camel they are organs of expression; and their office as organs of expression becomes more marked, and more extensive, as we advance through the cat, dog, and lion to the monkey. Farther, the order of the muscular system in fishes with respect to respiration requires no phrenic, spinal accessory, or external thoracic nerve; and in conformity with this order, none of these nerves exists. In birds, the spinal accessory and phrenic nerve are wanting, because there is no muscle corresponding in office with the mastoid or diaphragm. In the camel, there is no muscle corresponding in office to the mastoid. Many of its actions, indeed, are executed by a set of short muscles along the side of the neck, attached to the vertebræ. But its respiratory actions cannot be executed by these or any other muscle; and accordingly, the spinal accessory is wanting. Differences of the same nature may be observed among birds. The duck has very little power of expressing rage; and its portio dura is not larger than a cambric thread, and passes only to the skin under the jaw. But the cock expresses the passion of rage with considerable force; and here the portio dura is much larger, and is profusely distributed among the muscles of the neck. We may likewise take notice of another curious illustration of his views, in a criticism by Mr Shaw of Cuvier's comparative anatomy of the elephant. Cuvier states, that the trunk of the elephant is supplied only by branches from the fifth pair. Mr Shaw conceived, however, that the French anatomist must have been mistaken, as the researches of Mr Bell assigned to the facial branches of the fifth the power of conveying sensible impressions only; and in conformity with his preconceptions he discovered, that a branch of the portio dura, as large as the sciatic in man, joins the branch of the fifth which supplies the proboscis, and that the former is expended entirely on the muscles of that organ, while the fifth nerve is continued as a large trunk almost to the tip.

We apprehend, that the foregoing exposition of Mr Bell's doctrine, and of the evidence on which it rests, will satisfy the reader of the justice of the opinion we expressed at the outset. Yet at the same time it cannot be denied, that its final establishment is opposed by obstacles serious enough, although certainly not insurmountable. These obstacles relate rather to the details than to the principles of the doctrine, and exist at present in the form of doubtful, not of decided objections. We conceive it is conceded on all hands, that certain nerves must be intended for combining together all those muscles which execute the essential and collateral acts of respiration. This proposition, indeed, only requires enunciation, in order to receive immediate assent. The question then comes to be, whether such nerves are appropriated to that purpose only. Mr Bell has rendered it exceedingly probable that such is the single purpose of some of them; but he has not done so for all; and he has absolutely proved it, in our opinion, for none. The nerves, whose part in the respiratory system has been left by him in the greatest state of uncertainty, are the glosso-pharyngeal, par vagum, and the fourth nerve.

The next, and only other circumstance we shall mention at present, as requiring farther consideration on the part of our author, is the relation which the respiratory nerves and muscles bear to volition. We apprehend it to be his meaning, that the respiratory nerves do not belong to the apparatus of volition. This doctrine, indeed, is not always very distinctly adhered to throughout the work; but still it appears to be a part of the system he has constructed, and is laid down expressly in several places. It is a doctrine, however, which cannot be easily maintained as applicable to at least all the respiratory nerves.

In the *first* place, we do not admit it to be a proof of the combination of the various respiratory actions being involuntary, that they are habitual. For, on the one hand, other actions, decidedly the result of habit, are almost or altogether as unavoidable, when the ordinary cause of them is applied to the body; and, on the other hand, some of the respiratory actions which are usually combined, perhaps even all of them, may be abstracted by an effort of the will. Thus, under the influence of those emotions which cause the expression of smiling, many persons can preserve the countenance unmoved; or, when the essential organs of respiration are convulsed with the peculiar movements which accompany laughter, the corresponding movements of the countenance may be prevented altogether; or, when the respiration is laborious, the alæ nasi can be kept at rest and flaccid; or, in the action of yawning, the jaws may be prevented from gaping, the nostrils from being extended and curled up, the lips from becoming expanded, the eyelids from closing, the eyebrows from becoming depressed and corrugated,—and all this without any antagonist muscle being called into play.

But, *secondly*, although the habitual combination of various muscles, in certain respiratory acts, should be received as proof that the combination is involuntary, it still remains to be inquired, whether the respiratory nerves of Mr Bell are not capable of conveying the impressions of the will. It appears to us evident that they do, at least in some circumstances. Mr Bell has proved that the muscles of the face receive their muscular nerves only from the portio dura of the seventh, which is a respiratory nerve. Now, there is hardly a single muscle of the face which may not be made to contract, either alone, or in conjunction with others, by an effort of the will. The nostrils may be expanded or may be curled up, both eyebrows or one eyebrow may be raised or depressed, one eyelid may be closed, one side of the lip may be curled up or turned down, and so forth; and each and all of these motions may be executed by a pure effort of the will, quite unconnected with any essential or collateral act of the breathing, or with the expression of an mental emotion.

Having made these few strictures, for which we beg our author's attention when he completes and remodels his system, we must dismiss the subject abruptly. The notice this work has already universally received, makes any general recommendation of it on our part quite unnecessary. It is sufficient to say, that it places him, as a physiologist, in the same high rank which he has long held as an anatomist and a surgeon.

II.

The Croonian Lecture. On the Existence of Nerves in the Placenta. By Sir EVERARD HOME, Bart. V. P. R. S. Read November 18, 1824. Philosophical Transactions of the Royal Society of London for the year 1825, Part I. London, 1825.

SIR EVERARD HOME is beyond controversy a very great genius—great in comparative anatomy, great in physiology, and extremely great in philosophy. In each of these extensive and difficult departments of science he has achieved discoveries, which none but a consummate adept could in the least degree approach; and aided, as he has ever been, by the penetrating microscope of Mr Bauer, he has unlocked the most hidden recesses of nature, and revealed secrets which none but the most favoured votaries of science could presume to conjecture. But of all the marvellous feats which the illustrious Baronet has achieved, none will add more brilliant lustre to his reputation, than the one recorded in the Croonian Lecture before us. By the aid of the same invincible microscope, he has untied a knot which puzzled the patient and diligent inquiry of HALLER, excited the scorn and scepticism when it eluded the ingenuity of BUFFON, and was misunderstood in principle while it was believed in fact by our countryman LOCKE. To sustain the weight of discoveries so important, and so generally interesting, requires no ordinary strength of mind;—and if Sir Everard hears unmoved the ardent commendations and loud wonderment of his friends and admirers, we trust that the Royal Society will bear their faculties with that meekness and modest dignity which becomes a body so insensible to the extravagance of vulgar applause, when they are apprised of the panegyrics which are showered by the liberality of the scientific world on the researches of their indefatigable Vice-president.

Doubtless there are miscreants on this ungrateful globe who will have the assurance to say, that Croonian lectures are but dull and heavy things; that the facts which they contain are peculiarly liable to be questioned; that the conclusions which they announce are in many instances not consequences; that their attempts at reasoning savour strongly of gratuitous and independent assertion; and that their whole composition is so heterogeneous and desultory, that, unless the one half of the audience is asleep, and the other in a state of the most profound ignorance of the matter in hand, it is difficult to comprehend how the lecture can be heard to the conclusion. If such caitiffs actually live within the pale of a land which boasts of the existence of a Royal Society, and the institution of a Croonian Lecture, we recommend then *favete linguis*; that is, not to betray their ignorance and bad taste, by condemning that of which they are unfit to relish the merits. *Au reste*, the lecturer, is secure of the approbation of the Society; and why should he regard the contemptible murmurs of the profane vulgar, who have not been initiated into the mysteries of this temple of science? The hierophant of a shrine so sacred is much superior to the praise or the censure of ignorant neophytes.

That the subject of the researches of the learned Vice-president is entitled to the character here given, does not appear exactly at first sight; for the author has, with singular modesty, announced in the title the discovery only of nerves in the placenta. But, as in some instances of literary performances of the first and most transcendent merit, the modesty of the title forms a beautiful foil to the interest and importance of the substantial contents, so, in the case of the Croonian Lecture, the discovery of placental nerves is the smallest and least considerable of the information communicated by Sir Everard Home. It is the remarkable application which the ingenious author makes of this discovery, that gives the present paper its main interest, and to which, we confess, it is indebted chiefly for our attention. The question of the influence of the imagination of pregnant females over the fœtus, which derived its greatest importance from the proportion of the marvellous which entered into its composition, and which has been doubted and denied, or maintained and vindicated, very nearly in proportion to the knowledge and ignorance, or credulity of the parties, has at length been erected into a physiological doctrine by Sir Everard, and been indissolubly fastened to the functions and properties of the nervous system. A subject which has received attention so marked from Sir Everard Home ought not to be treated lightly or carelessly by others; and it may not be improper to inquire into the actual merits of a doctrine, on which so much contrariety of opinion has prevailed. We commence the train of our observations somewhere in the middle, with the express view of attracting attention.

Father MALEBRANCHE has some curious observations on the communication which he supposes to exist between the brain of a pregnant woman and that of the infant in her womb; and he applies the doctrine of this communication to explain the general resemblance which the offspring bears to the parent, and the particular phenomena which are occasionally observed in monstrous births. His ideas, it is true, on the operations of the brain, and especially of the faculty of imagination, are somewhat gross and mechanical; and although Malebranche was a disciple of the Cartesian school, and the great apostle of its tenets, this part of his philosophy has not only a strong savour of materialism, but gives a picture of mental operations so grossly mechanical, that it may now be the subject of astonishment how any one could have the assurance to fabricate, or the credulity to believe, principles so gratuitous and imaginary.

After remarking that infants in the womb not yet entirely formed, *must* be connected with their mothers in the most intimate manner possible, he infers that they have the same sensations, and the same passions, in one word, all the same thoughts which arise in the soul after corresponding motions of the body; —that they see what their mothers see, hear the same cries, receive the same impressions of objects, and are agitated by the same passions. The body of the infant he regards as the same with that of the mother, the blood and spirits common to both; the sensations and passions, the natural consequences of the motion of the spirits and of the blood; and these motions necessarily communicated from the mother to the infant. In short, the passions, the sensations, and in general all the thoughts of which the corporeal frame is the occasion, are, according to him, common to the mother and to the infant.

Though the learned Father informs us that these things appear incontestable to him for several reasons, yet we have no hesitation in declaring that these reasons are not satisfactory to us; and that no reasons whatever are adequate to establish opinions so imaginary, and a theory of communication between one part of the animal body and another, in the process of production by it, so completely dependant on the will of its author. The whole theory, in short, is so pregnant with absurd and gratuitous assumptions, that it carries its refutation along with it, and is in no danger of ever being seriously believed by any one who knows any thing either of mental or of corporeal operations, as they take place in the human body. Yet, notwithstanding the validity of these objections, several of the facts from which he draws his conclusions are so well established, that if his explanation be inadmissible, it is impossible to deny entirely the phenomena from which it has arisen; and however frivolous or ludicrous the inquiry may seem, it has been regarded as important enough to attract the attention of some of the most illustrious persons who have cultivated the science of physiology, and among others of Buffon, Haller, and Morgagni.

Malebranche, who appears in his philosophy to have been the mildest and most amiable of Catholics, was evidently aware of the weakness of his theory, and the strength of his facts; and though he yields up the one with the most perfect good humour to the tender mercies of his readers, he cannot resist the temptation of giving them a good hard morsel of the other. "You may regard," says he, "what I have now said, as mere supposition, if you choose; but you should understand it well, if you would form a clear conception of the matters which I am going to explain in this chapter. For the two suppositions which I have now made, form the principle of an infinity of things which are generally believed very difficult and very obscure, and which appear to me indeed incapable of explanation, without admitting them. Here are examples."

"About seven or eight years ago, there was at the Hospital of Incurables a young man, an idiot from birth, in whose person were fractures in the same places in which criminals are broken on the wheel. In this state he had lived for twenty years, during which he had been seen by several persons; and the late Queen-mother having gone to visit the Hospital, had the curiosity to see him, and touch his arms and legs at the places where they were fractured. According to the principles which I have above established, the cause of this fatal accident was, that his mother, when pregnant, knowing that a criminal was to be broken on the wheel, went to see the execution. Every blow inflicted on the miserable felon, forcibly struck the imagination of the mother, and by a sort of counter-stroke, the tender and delicate brain of the infant. The fibres of the brain of the mother, though dreadfully shocked, and perhaps ruptured in certain parts, were, however, sufficiently consistent to withstand entire dissolution. Those of the brain of the infant, on the contrary, unable to resist the commotion of the spirits, were entirely sundered; and the ravage was so great as to deprive him of reason ever after. Further, at a sight so dreadful, the animal spirits of the mother rushed violently from her brain to all the parts of her body corresponding to those of the criminal; and the same thing took place in the infant. The bones of the former, able to resist the violence of this impulse, were not injured; while those of the latter, which were soft and tender, were unable to withstand, and were ruptured." *

This explanation, which we quote, not for its merit or philosophical accuracy, but for its curiosity and extravagance, is so exceedingly absurd and ridiculous, that it is difficult to say whether it shows more clearly the low ebb of physiological knowledge in the days of this acute writer, or the gross materialism with which the philosophy of the human mind was then contaminated and degraded. Such as it is, however, it is seriously given by an author who, to most of the knowledge of the natural sciences then attainable, added a capacity for metaphysical inquiry, and a range of intellectual speculation, which renders him still a high authority among writers on metaphysical subjects. But notwithstanding this unfavourable specimen of the physiological attainments of Father Malebranche, we must venture on another, to which he applies the character of "*un exemple assez particulier.*"

"Not quite a year ago, a woman, who had gazed too attentively on the painting of St Pius, during the celebration of the festival of his canonization, was delivered of an infant who entirely resembled the image of the saint. He had the countenance of an old man, as far as an infant without beard is capable. The arms were crossed on the breast, the eyes raised toward heaven, and the forehead was very small,—as in the painted representation of this saint, which is raised towards the cieling of the church in looking toward heaven, there is almost no forehead. He had the figure of a mitre reversed on his shoulders, with several round marks on the places where the mitres are covered with jewels. In short, this infant entirely resembled the painting, on which his mother formed him by the force of imagination. It was a thing seen by all Paris, as well as by myself, because the body was long preserved in spirit of wine, and is perhaps so preserved still." The explanation of this story is, as may be supposed, so similar to that of the last, that our readers will dispense with the repetition of it.

But however frivolous and ridiculous the manner in which the holy Father has treated this subject may appear to modern readers, it requires very little reflexion to perceive, that it differs not very materially from the opinions of several modern physiologists; and that the change consists rather in a change of names, than of ideas and principles.

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* De la Recherche de la Verité, tom I. Livre II. chap. III.

——mutato nomine de te
Fabula narratur.

The object of Malebranche was to explain a doctrine of great and remote antiquity on the principles of the Cartesian philosophy, and by the application of the anatomical knowledge and physiological hypotheses of the day, to demonstrate the mode in which the objects of sensation produced, through the medium of the brain and nerves, certain effects on the *fœtus in utero*; and as he was on the one hand presenting old doctrines under new names, so on the other, his speculations have been adopted with a slight variation of terms by many subsequent inquirers. The truth is, that, though the idea of the influence of the imagination of the mother over the shape and appearance of the offspring, is one of the most ancient and firmly rooted tenets of popular belief, yet it is not only uncertain whether the faculty denominated fancy or imagination has the powers ascribed to it, but whether under any circumstances it can produce those effects which have been traced both by philosophers and by the vulgar to its operation.

That the belief in the influence of this power is of very high antiquity, appears from the account preserved in the Sacred writings of the contrivance which the patriarch Jacob is represented to have employed, in order to augment the number of calves, lambs, and kids, which, according to the agreement between Laban and him, were to fall to his share.* For even in admitting the agency of Divine power in giving the particular colours to the offspring, the mere circumstance of the contrivance said to have been practised by the patriarch, is proof sufficient that it was a popular belief in the patriarchal times, and in the days of Moses, that the contemplation of particular objects, by breeding animals, might influence the colour of the offspring. It was not to be expected that at this early period mankind would attend minutely to the operation of the senses, or to their subsequent effects on the processes going on in the animal body; and it does not appear that the channel through the medium of which these effects were believed to take place, was yet distinguished as a separate faculty, or had received any appropriate denomination. The ideas of mankind on such subjects must have been vague and ill-defined at this period; and notwithstanding the conjectures of the learned Brucker, on the principle by which the

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† Genesis chap. xxx. 37–39.

contrivance was supposed to be dictated, it is manifest that we possess too few facts, and these too imperfect, regarding the ancient Hebrew philosophical notions, to speak with confidence concerning it. Without attempting to collect the uncertain and scattered traces found in the Egyptian history, we proceed to remark, that in Greece, which was the nurse, if not the parent, of all philosophy whether true or false, we find pretty ample proofs of the extent of this belief in the state of the mental feelings over the process of conception. In this, however, as in many other things, the poets have preceded the professed philosophical writers; and it is in the *Works* and *Days* of HESIOD that we find the first traces of its existence.

> Μηδ' ἀπο δυσφημοιο ταφου ἄπονοστησαντα
> Σπερμαινειν γενεην ἀλλ' ἀθανατων ἀπο δαιτος.

> "Nor seek to taste her beauties, when you part
> From a sad funeral with a heavy heart;
> But when from joyous feast you come all gay."—COOKE.

The popular opinion was at length sanctioned by the authority of HIPPOCRATES and GALEN. In one of the writings ascribed to the former, the author admits that fetal spots or deformities may arise from the capricious desires of the pregnant mother; * and he appears to have on one occasion rescued from punishment a woman of rank, who, though fair herself, had given birth to an Ethiopian infant, on the ground that, during pregnancy, she had been in the habit of gazing at a painting of that colour. That this idea was not confined to physicians, appears from the testimony of GALEN in his Epistle to PISO, in which he remarks, that the ancient Greeks and Romans, in order to have beautiful children, took special care that the mothers should behold agreeable paintings only, beautiful statues, and other objects fitted to excite the most pleasing and delightful mental images. "Further," says Galen, "I learned from an ancient writing, that a certain person, deformed but wealthy, desirous to beget a well-formed infant, caused to be delineated on a board the figure of a comely child, on which he commanded his wife (τῃ γυναικι συνπλεκομενος, *inter coeundum*) to gaze; and so stedfastly did she fix, not only her eyes, but I may say her very soul, on the lineaments of this figure, that she gave birth to an infant which resembled not the father, but the painting entirely, her vision having, in my opinion, transmitted the impressions of the latter." *

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* Ἢν τισι κυισκουσι γην ἐπιθυμηται ἐσθιεσθαι ἤ ἀνθρακας, και ἐσθιωνται, ἐπι της κεφαλης του παιδιου φαινεται ὁκοταν τεχθη σημειον απο των τοιουτων. Hippocrates περι επικυησιος κεφαλ. ϛ.

A more remarkable instance still of the prevalence of this opinion, forms the basis of a very curious work, the Ethiopics of Heliodorus, almost the only romance of antiquity. According to the account given by this author, Chariclea, the heroine of the story, though perfectly white, is the daughter of Hydaspes and Persine, king and queen of the Ethiopians, both of the deep black which distinguishes that race. The mother, for various reasons, commits the daughter, with certain tokens, to the care of Sisimithras, a confidential gymnosophist, who conveys her, at the age of seven, to the court of Oroondates, and delivers her to Charicles, priest of the Delphian oracle. Chariclea, after a variety of adventures in various countries, is at length restored to her parents in Ethiopia, where Sisimithras the gymnosophist, to whom the infant had been originally entrusted, after relating her history, explains the peculiar colour, by ascribing it to the influence of a figure of Andromeda, on which the mother Persine was in the habit of gazing. "As to the question you put about the lady's complexion, the fillet has given you a true answer, namely, the sight of Andromeda's picture, which possessed the queen's imagination during your amorous congress. Of which, if you desire further confirmation, let the picture be sent for, and you will find the young lady an exact copy thereof." † It is evident, that, though the courtly gymnosophist is here made to echo the account formerly inscribed by Persine on the fillet, yet the story being thus interwoven by Heliodorus into a long narrative, is a clear proof of the general extent to which the belief was adopted.

The strongest and most circumstantial proofs, however, of the existence of this opinion among the ancients, are found in the writings of Oppian, the poetical Izaac Walton, and Gervase Markham—of the Romans,—the panegyrist and connoisseur

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* Ἐμοι δε και λογος τις αρχαιος εμηνυσεν ὁτι των ἀμορφων τις δυνατος, ἐυμορφον θελων γεννησαι παιδα, ἔποιησε γραψαι ἐν πλατει ξυλῳ ευειδες αλλο παιδιον, και ελεγε τῃ γυναικι συνεπλεκομενος εκεινω τῳ τυπῳ της γραφης ἐμβλεπειν· ἡ δε ἀτενες βλεπουσα, και ως ἐιπειν, ὁλον τον νουν ἔχουσα, ὀυχι τῳ γεννησαντι, ἀλλα τῳ γεγραμμενῳ ὁμοιως ἀπετεκε το παιδιον, τῆς οψεως, ὀιμαι, διαπεμπουσης τῃ φῦσει, ἀλλ᾽ ὀυκ ὀγκοις τισι του γεγραμμενου τους τυπους.

† Της γε μὴν κατα την χροιαν ἀποριας φραζει μεν σοι και ἡ ταινια την λυσιν ὁμολογουσης, ἐν ἀυτῃ ταυτησι Περσινης εσπακεναι τινα ειδωλα και φαντασιας ὁμοιοτητων απο τῆς κατα την Ανδρομεδαν προς σε ὁμιλιας ὁρωμενης· ειδ᾽ουν και αλλως πιστωσασθαι βουλει προκειται το αρχετυπον, επισκοπειν την Ανδρομεδαν ἀπαραλλακτον ἐν τῃ γραφῃ και ἐν τῃ κορῃ δεικνυμενην.—*Heliodori Ethiopic. lib. x.*

of the noble sports of Fishing and Hunting. The Spartans, according to the account of this lover of field-sports, were remarkably attentive to the breed of their human animals, and, with the view of securing a well-formed and robust offspring, after proper attention in the selection of the parent, their next care was to cause their pregnant women to gaze on excellent paintings or figures of Nireus, Narcissus, and Hyacinthus, and of such pugilistic champions as Castor and Pollux, and the more youthful and handsome of the divinities. In the same manner the poet shows how careful experienced breeders of horses are in attending to the imagination of the dam, and the solicitude of fowlers in having a beautiful race of birds.

Ἡ δε ὑποκυσσαμενη πολυανθεα γειναтο παῖδα,
Νηδυς μεν ποσιος γονιμον θορον ἀειρασα,
Δεξαμενη μορφην δε πολυχροον ὀφθαλμοισι.

Tum mater gignit maculosum corpore pullum,
Semine fæcunda sponsi matrice recepto,
Concipiens oculis objectos mille colores.

Ναι μην ὧδε Λακωνες επιφρονα μητισαντο,
Αἴσι φιλαις ἀλοχοις, ὅτε γαστερα κυμαινωσι,
Γραψαντες πινακεσσι πελας θεσαν ἀγλαα καλλη,
Τους παρος ἀστραψανταις ἐν ἦμεριοισιν ἐφηβους,
Νιρεα, και Ναρκισσον, ἐΰμελίην Υακινθον,
Καστορα τ᾽ευκορυθον, και Αμυκοφονον Πολυδουκην,
Ἠΐθεους τε νεους, τοι τ᾽ εν μακαρεσσιν ἀγητοὶ,
Φοιβον δαφνοκομην, και κισσοφορον Διονυσσον.
Αἱ δε ἐπιτερπονται πολυηρατον εἶδος ἰδουσι.
Τικτουσιν τε καλους ἐπι καλλεϊ πεπτηυιαι.

Ista solent etiam sæpe usurpare Lacones,
Dum uxores cernunt utero catulire tumenti.
Eximie pictas tabulas, et splendida ponunt,
Quæque hominum genera effulserunt inter ephebos,
Nirea, Narcissum, bellatoremque Hyacinthum,
Castora cristatum rutilanti casside ferro,
Pollucem qui Amycum valuit superare palæstra,
Atque adolescentes facie membrisque decoros,
Egregiosque deos, qualis Thymbræus Apollo
Conspicua lauro redimitus, qualis et Evan
Cingens pampineo fusos diademate crines.
Dum tabulas lustrant oculis formasque venustas,
Attonitæ pariunt formosos ore Lacænæ.

These proofs apply, however, chiefly to the objects of external sensation, and especially vision. From what cause was it that the alleged effects came to be ascribed to the operation of fancy? When the example of Plato and Aristotle directed the acute and reflective spirit of their countrymen to carry their inquiries beyond the objects of external sensation, they supposed a particular faculty to which they ascribed the power of recalling and modifying the images of such objects, and to which, by a natural analogy to what is observed in sensible objects, they applied the name of *phantasy* or *fancy*. The Stagyrite had previously defined phantasy to be an animal motion flowing from the acting sense, by which motion the animal is variously affected, conceiving things sometimes true, sometimes false; and he represented deliberative fancy as peculiar to men, enabling them to choose the best, and subduing the sensitive, which is common to brutes also. This dogma was indeed the commencement of the hypothesis of the power of the imagination; but it was the commencement only. It was in the school of the New or Third Academy, and under the auspices of the philosopher CARNEADES, that this faculty appears first to have acquired that distinct existence and that sovereign power over the actions of the man, which it has ever since held by a sort of undisputed and hereditary authority; and it is undoubtedly a mortifying, though not an uncommon thing, to perceive, that the influence of an eloquent and sophistical teacher has been capable of creating and perpetuating errors in the natural history of the intellectual powers, which still continue to operate in retarding the progress of knowledge and the discovery of truth. According to the account of SEXTUS EMPIRICUS, to whom we are indebted for a tolerably clear statement of the principles of Carneades, phantasy is defined to be "a certain affection in an animal, which shows both itself and another object in such manner, that when we behold any object, we are in some sort affected by the sight thereof; nor does the sensation continue the same as before the affection,—a change, which demonstrates two things; first, the change itself, that is Fancy; secondly, the object which caused the change, or what was seen." He afterwards infers, from various phenomena, that Fancy is the imagining of any thing; namely, of that object from which the process takes place, and of that in which the process takes place; the sensible object in the one case, the sentient or percipient agent in the other.

These ideas, though by no means precise or distinct, are perhaps as much so as the nature of the subject admitted. Though they observed and remarked the effects of sensation and of its objects on the ulterior operations of the mind, it was utterly impossible that they could know the link by which they were connected;—much less could they trace the reflex operation of the mental powers on the corporeal frame. What was fact therefore, or matter of observation, they were competent to record and state; but whatever was beyond the sphere of sensible testimony, necessarily became the subject of supposition; and that supposition was most natural which deviated least from the course of phenomena already learnt in the observation of material and sensible objects. It cannot be doubted, that both the original name by which this faculty was distinguished, and the ideas attached to it by the philosophers of the New Academy, indicate that this was the origin of the faculty of phantasy or imagination; and while the very nature of the subject made them substitute conjecture and supposition for fact and demonstration, the little acquaintance which they had with the nature of mental operations in general, and the erroneous or imperfect mode in which they studied these operations, powerfully contributed to the prevalence of the fallacious views which they took, and the confused ideas which they entertained.

Nor was the Stoical sect more fortunate in elucidating this obscure subject, or correcting the errors which had arisen from the example of Carneades; and even so late as the age of Cicero, who was solicitous to expound to his countrymen the niceties of the Greek philosophy, much the same language and the same ideas prevailed on the nature and powers of the imagination as in the days of Aristotle and Carneades.* And it was in allusion to this obscurity that BOETHIUS, some centuries after, wrote those verses, in which he gave the philosophers of the portico the character of delivering unintelligible opinions on the operations of the senses, and the manner in which they conceived or imagined the images of objects to be impressed on the brain.

Quondam porticus attulit
Obscuros nimium senes
Qui sensus et imagines
E corporis extimis
Credant mentibus imprimi
Ut quondam celeri stilo
Mos est æquare paginæ
Quæ nullas habeat notas
Pressas figere literas. †

Obscure, however, and confused as these ideas must appear to modern readers, and absurd as they must appear to those who have ever adverted to the physical impossibility, as well as the complete want of demonstration of this theory, it is easy to trace to them the origin of that belief which ascribes so much power to the imagination in the formation of the fœtus. In the earlier ages, before men had begun to philosophize on the nature and operations of the mind, they appear to have ascribed the effect simply to vision, or beholding the objects; but as they advanced in refinement, the abstruse philosophy of the academy was put in requisition; and it was towards the latter ages of Greek philosophy that this opinion began to make part of the current principles of the day, and that effects which were previously ascribed to mere external sensation, were referred to the operation of that power by which the mind receives and modifies the images of external objects. Thus, though the language used by Hesiod and Hippocrates manifestly refers to some emotion or affection of the mind, Galen and Oppian speak distinctly of mere vision; and though it may be argued, that this implies the operation of fancy, they nevertheless do not appear to have decidedly gone further than the mere admission of the power of the exterior sense. Heliodorus, on the other hand, expresses himself more unequivocally, and uses the terms ειδωλα and φαντασια, *imagines* and *imaginatio*; and Soranus, as quoted by St Augustin, expresses himself much in the same manner.

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* Marci Tullii Ciceronis Questionum Academicarum, lib. i. c. 11.

† De Consolatione Philosophicae, L. V. metr. 1. p. m. 233.

From the ancient physicians and metaphysicians this theory was adopted first by the doctors of the Arabian school, especially Avicenna, then by the schoolmen of the twelfth and thirteenth centuries, and afterwards by the physiological writers of the fifteenth and sixteenth centuries, when it appears to have been both generally and implicitly admitted. But if it was founded in error, and blended with absurdity among the ancients, it underwent a much more thorough contamination at the period termed the revival of letters. Judicial astrology was then eagerly cultivated; a belief in sympathetic powers or occult qualities and unseen connections of different objects was generally entertained; and a not less powerful agent was found in gross superstition, the invariable companion of ignorance. With these pernicious ingredients the original opinion of the ancient philosophers was blended and disguised; and, the credulity of the age favouring its dissemination, marvellous and extraordinary examples of the power of fancy in pregnant women were accumulated and related to gratify this vicious appetite.

Among the scholastic doctors of the thirteenth century, ALBERTUS of Bollstaedt, a Dominican monk, better known by the name of ALBERTUS MAGNUS, * deserves particular mention for his exposition of the doctrine of imaginative power on pregnant women, which he contended was originally derived from celestial influence. He was indeed a firm believer in judicial astrology, and enters into a very lengthened discussion on the influence of the planets on the *fœtus in utero.*

"In his book on minerals, he represents this imaginative faculty, which is the operator of effects, as derived from heaven;

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* Born at Lawingen on the Danube in 1207.

he asserts that heaven, and not the imaginative faculty, produces the effect; and that, when heaven has any effect to produce, then also it moves the imaginative power, by the medium either of sense or of intellect; and that, whatever is the cause of the cause, is the cause of the thing caused. And in this manner, according to Albertus, the imaginative produces any effect; and thus, from the imaginative, the form becomes universal and particular in the fœtus. Yet in my judgment, says Berenger, it is requisite that the matter be fit to assume such forms, because, as the same Albertus says of stones, the matter of which is terrestrial and gross, *images* are not formed, on account of the unfitness of their matter." [*]

These mysterious and extravagant notions were not only imbibed by his pupil, the celebrated Thomas Aquinas, the Angelic Doctor; who made of them a most unintelligible and imaginative mixture with universals and substantials, elementary qualities and formative principle; but were adopted and defended by our countryman Roger Bacon, Dino de Garbo, Professor of Medicine in Bologna, Bernard Gordon of Montpellier, Arnoldus de Villa Nova, and in short, by most of the eminent scholars and physicians of the age. When there was so little real knowledge of the material world and of natural processes, the chief means of distinction between philosophers and the uneducated was absurdly enough supposed to be these extravagant pretensions to secret and incomprehensible knowledge.

Of this vain reasoning and false philosophy, it is impossible to give a more appropriate example than what we find in the curious writings of JACOPO BERENGARIO DE CARPI, commonly named *Carpensis*, the learned commentator on the writings of MONDINI, the founder of the science of human anatomy. And as Berenger was careful to collect every thing that was then known or believed, his observations may be justly regarded as a fair average specimen of the philosophy of the day on this subject. After enumerating various circumstances that may influence the growth, shape, and appearance of the fœtus, Berenger informs us, that Dinus de Garbo, a great authority with him, asserts that the cause of the infant resembling neither father nor mother, nor any progenitor, may be twofold, one in part the imagination, because according to the authority of Avicenna and others, a white woman had born a black child, *quia vidit quandam picturam in camera dum haberet rem cum viro, et ex pictura fuit imaginata taliter qualiter deinde peperit filium nigrum.* Shortly after Berenger alludes to the story of the patriarch and the party-coloured lambs and kids; and to show that his countrymen were not outdone in ingenuity, he adds, that in his own times even, skilful shepherds were wont to place before their ewes objects of the colour which they wished in the lambs.[*] "In short," says Berenger, "that the imaginative power contributes greatly to the individual shape, is proved by this, that pregnant women, if they imagine, by a strong effort of imagination, any fruit or other eatable article for which they long, whether their appetite be sick or sound, and whether their imagination moves from intellect or from sense; and if they touch their person any where;—then, from such imaginative power of the mother, is introduced the form of the wished-for object into the members of the fœtus."[†]

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[*] "Albertus in libro de mineralibus dicit quod "illa imaginativa quæ est effectuum operatrix est mota a cœlo, et dicit quod cœlum est illud quod facit illum effectum et non imaginativa; sed dicit quod dum cælum habet aliquem effectum producere, tunc etiam cœlum movet imaginativam aut mediate sensu aut intellectu; et sic quicquid est causa causæ est causa causati; et isto modo secundum eum imaginativa producit aliquem effectum; et ita ab imaginativa fit forma in fœtu universalis et particularis. Requiritur tamen judicio meo quod materia sit apta suscipere tales formas, quia ut ipse Albertus dicit in lapidibus quorum materia est terrestris et grossa, non fiunt imagines propter ineptitudinem materiæ."—Albertus apud Jacobi Berengarii Carpi Commentaria in Mundinum. Impressum Bononiae per Hieronymum de Benedictis, 1521. Folio 274.

These proofs from the writings of the Professor of Surgery in the University of Bologna, in the beginning of the sixteenth century, may communicate a very just notion of the mode in which physicians then thought and reasoned on the power of imagination in pregnant women. But hitherto they had only borrowed and expanded the doctrines of the ancients.

Berenger not only adopts the astrological opinions of Albert, already noticed, and compares them with those of Nicolas, but enters into a long and somewhat abstruse inquiry into the particular mechanism by which the effects on the fœtus are supposed to be produced. But the point to which he attaches most importance is, why the mark appears on that part of the body of the infant, which corresponds to that which the mother touched in her own person, and no where else; and of this he gives three solutions. *First*, According to some, the fœtus, which is a part of the mother, is soft and incomplete, and therefore easily receives an impression, which the perfect and solid frame of the mother neither receives nor retains; *Secondly*, According to the celestial physiologists, the heavenly influence which introduces the mark into the fœtus, through the medium of the formative virtue, excites the imagination of the mother, and leads her to apply her hand to that part of the person in which the mark will appear in the fœtus; *et ista ratio mihi placet*, he says; and, *Thirdly*, These marks depend on celestial influence operating through the medium of zodiacal aspects; *et hoc etiam credo*, says Berenger.[*]

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[*] "Temporibus etiam nostris pastores periti id agunt et illum colorem quem volunt in nascentibus ante oculos ovium ponunt."

[†] "Et quod etiam imaginativa multum conferat ad formam individualem probatur, quia mulieres prægnantes si imaginantur forte imaginatione aliquem fructum vel aliam rem comestibilem quam appetunt, sive appetitus earum sit morbosus sive sanus, et sive imaginativa sua moveatur ab intellectu sive a sensu, et quod tangant corpus suum in aliqua parte, tunc a tali imaginativa matris introducetur forma talis rei appetitæ in membris fœtus."

We believe we may justly say, with this diligent and not illiterate, but very credulous author, *multa alia de hoc dicuntur, sed hæc pro nunc sufficiunt.* From the specimens now exhibited, a true and correct idea may be formed of the opinions and reasoning, if reasoning that may be called where reason there is none, of the physiologists and physicians of the thirteenth, fourteenth, and fifteenth centuries. So generally was this belief diffused, that it is more difficult to find exceptions than examples of its prevalence among the authors of the day; and so slavishly does it appear to have been adopted, either from the astrological or the scholastic authorities, that the only instance in which a slight doubt of its truth can be remarked, occurs in the writings of Nicholaus Rochaeus, a physician-accoucheur, whose treatise on the diseases of women is preserved by Israel Spach. We find it, on the contrary, maintained in various degrees of extravagance during the 16th century, in the writings of Jason Pratensis, Eucharius Rhodion, and John Langius in Germany, Jacobus Ruffius or Rueff, in Switzerland; by Bernard Felician, Ludovico Bonaccioli, Michael-Angelo Biondi, Lactantio Eugenio, Hieronymo Mercuriali, Luigi Septala, and Marcello Donato, in Italy; by Luis de Mercado, in Spain; by Ambrose Paré, and Andrew du Laurens, in France; besides such enthusiasts as Paracelsus and Cardan, and a multitude of minor names too numerous to recount. We cannot enter into the full history of this mortifying instance of the weakness of human judgment, and the ridiculous errors into which men of no ordinary talents, when left to the wanderings of their fancy, are liable to be betrayed; but it may be neither useless nor uninteresting to show a few examples of their folly.

Ludovico Bonaccioli (1510) asserts, that he has ascertained the influence of imagination during pregnancy by experiment;[*] yet he has the sense to admit that many of the stories of monstrous births may cause wonder, though very devoid of credulity. Jacobus Ruffius of Zurich, who, however, has written a very good chapter on the facts, then known, regarding monstrous and imperfect births, admits the operation of fancy, and gives rules to prevent its effects.

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[*] Folio 274, 275.

[†] Quin imo præter haec, similitudinum quoque reputationem in mentem esse, utpote in qua multa fortuita pollere credantur, memoria rerum quae visae auditaeve fuere, imaginibus sub conceptu haustis, compertum experimento habemus. In Israelis Spachii Gynæcacorum Libris p. 146.

In 1568, Lactantio Eugenio, a physician of Narni in Umbria, published a small treatise *de maris et femellae generatione*, in which, among many other strange and chimerical notions, he revived the doctrine of the ancients, and with the view of reducing it to practice, recommended the imagination to be occupied *inter coeundum*, with the images of the most beautiful and perfect specimens of the human form, which he contends is a never-failing[1] means of procuring a beautiful offspring. Hieronymo Mercuriali, though he admits the susceptibility of pregnant women, is, however, so philosophical as to doubt whether capricious appetites and desires can communicate marks or cause deformities in the bodies of their infant offspring. Yet, because the inquiry is difficult, and he sees no better mode of explaining the phenomena, he slavishly resigns his faith to the dictates of Ebn-sina.[*] Among the Italian physicians of the sixteenth century, who were wedded to the imaginative theory, must also be numbered Luigi Septala of Milan, who, in a treatise, *De Nævis*, defends it strenuously, and contends for the secret or invisible connexion between meteors and other heavenly bodies and the human frame; and Marcellus Donatus of Mantua, who devotes the Second Book of his Historia Medica Mirabilis to the exposition of the marvellous powers of the faculty of imagination. After these fooleries of the learned and rational Italians, it is impossible to wonder at the absurdity of Luis de Mercado of Valladolid, who, on the authority of Albert the Dominican and Thomas Aquinas, admitted the influence of the imagination in its fullest extent, and is moreover very partial to the theory of sidereal power delivered by the former.

Much about the same time, we have a specimen of the prevalence of this opinion among the medical authorities of France, in the writings of Ambrose Pare and Andrew Du Laurens, professor of medicine, and first physician to Henry IV. That the sound judgment of Paré was so much biassed by the spirit of the age as to admit the hypothesis in its strongest and most revolting forms, must be ascribed to the force of habit and the depth of early impressions; and his chapter on black infants, sprung from fair parents, and of the hairy progeny of smooth mothers, will remain a memorable proof of the credulity of the most rational minds, when clouded by superstition, and warped by prejudice. Not less may the sense of Du Laurens be questioned, of whom it has been justly said, that he was more illustrious by the offices which he held, than by the writings which he has left to posterity. In the anatomical history of this physician, published in 1595, in his eagerness to explain every thing, he gives a theory of monsters, in which he ascribes to the imagination of mothers the spots and deformities with which infants come into the world; and lest the reader should be incredulous, he quotes several examples, and among others, the hairy girl, who makes such a figure in the writings of SCHENKIUS, PARE, DONATUS, and others, and who was said to have been indebted for this alleged covering to a painting of St John the Baptist in a goat's skin, with which the imagination of the mother was indelibly impressed. To close this sketch of the imaginative theory of the sixteenth century, we look homewards, where we find a countryman, DAVID KINLOCH, distinguished for the sister gifts of medicine and poetry, delivering it in a poetical treatise *De Hominis Generatione*, in Latin Hexameters.

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[*] Est hoc in loco contemplatio non inutilis, an ubi gravida vehementer appetit aliqua, consueverit signari fœtus; nam videbitis plurimos qui habent signa, alii stigmata appetitus materni; circa quam contemplationem multi laborarunt, ut causas veras invenirent; sed certe res est difficilis, neque meliorem rationem ullam video quam quæ ab Avicenna affertur;—quod ubi mulier aliqua vehementer appetit, illud semper animo voluit, quo fit ut spiritus ipsi rei desideratæ forma figaret. —*De Morbis Muliebribus. Lib. I. Chap. IV. de Gravidarum Cura.*

> " Fixa tamen quandoque modis meditatio miris
> Matris materiam immutat. Sic casta nigello
> Dicitur Aethiopum quondam regina marito
> Aequantem candore nivem peperisse puellum,
> Alba quod in coitu turbassent spectra cerebrum." *

But in addition to this view of the imaginative theory among the regular and professed physicians, the sixteenth century has to boast of the origin of a sect of fanatic and mystical enthusiasts, who pushed it, if possible, to a still more extravagant and incomprehensible degree of refinement. It will readily be understood that we allude to PARACELSUS and his followers, the brothers of the Rosy Cross, and of the Cabalistical signs. Before the time of Paracelsus, attempts had been made to unite magic and astrology to physiology, by HENRY CORNELIUS AGRIPPA, and we have already seen something of the same nature, in the ideas of Albertus Magnus, Roger Bacon, and Septala. Paracelsus attempted a different course, by looking for the explanation of physiological phenomena in the occult qualities of bodies, and in the knowledge of the signs, impressed by celestial intelligence on all matter. It was not to be expected that a subject so suitable was to escape the penetrating theory of this transcendant genius. According to him, the will and the imagination of man act principally by means of the spirit, which furnishes the explanation of sorcery and magical effects. Birth-spots are impressions of these *vicarious* men, and Paracelsus terms them in his affected jargon *cocomica signa*; the *syderic* body of man attracts to him, by imagination, whatever surrounds him, and especially the stars, on which it acts like a magnet. In this manner pregnant women, and those under the menstrual discharge, if their imagination be diseased, may not only poison a glass by their breath, and injure the infants in their womb, but even poison the moon. Their eyes are like those of the basilisk, which shoot death in every direction.

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* Davidis Kynalochi de Hominis Procreatione, lib. 1. Paris, 1591.

Whether CARDAN had any personal knowledge of these extraordinary opinions or not, it is certain that they agreed very close with his own. They were propagated with his other principles, by Leonhard Thurnheysser, Adam de Rodenstein, Gerard Dorn, and Peter Severinus, and several others. Then followed the mystical opinions of the Rosicrucians, with their internal or astral man, to which, in their abstruse language, the name of *Gabalis, genius, or imagination*, was given, which, according to them, attracts visible objects like a magnet. Then came VAN HELMONT with his Blas, his Archaeus, and imaginative issuing from the spleen, his magnetic force, his sympathetic media and influential power of the stars. By the magnetic force, he explains why the imagination of a pregnant woman, who longs for a cherry, if she scratch her forehead, impresses indelibly on the forehead of the fœtus, the figure of this fruit, which annually becomes green, yellow, and red, according to the season and the tree.[*] To sympathetic media he has recourse to prove, that the imagination may operate, and does operate at distances, like the glance of the basilisk or the shock of the electrical eel.[†] In the bold conceptions and figurative language of this doctrine, it is easy to discover the germs of the theory of animal magnetism, with which, from Mesmer to Deleuze, the world has been at once amused and hoaxed.

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[*] "Atque opportune æque lucide magnetismum sui operationem in distans confirmat signum a prægnantis appetitu fœtui impressum. Esto scilicet gravida quæ cerasum appetat alienum, digito sibi frontem scalpat. Extra dubium, fœtus in fronte signatur cerasi imagine, quæ deinceps singulis annis viridescit, albescit, flavescit ac tandem rubet, juxta tenorem arborum. Et adhuc multo mirabilius easdem vicissitudines maturitatum exponit; quod idem fœtus in Hispania (ubi cerasus floret sub finem Februarii) imitatus erit præfatas cerasi scenas longe citius quam penes nos."—VAN HELMONT, *Ortus Medicinæ. Vis Magnetica, p.* 489.

[†] "Quod autem conceptæ imagines in distans ferantur probat pregnans manifeste. Quippe quæ suorum conceptum idea illico omnes transfert in fœtum a matre

It was about the beginning of the seventeenth century (1608) that Thomas Fyens or Fienus of Louvain, published his treatise on the powers of the imagination, in which he collected the different cases recorded by authors, or what had come to his own personal knowledge,—a sort of summary of all that was known on the subject. It is but justice, however, to acknowledge, that however credulous he appears to be, he shows no tendency to rely on the visionary notions which have been already noticed, and ascribes the whole of the phenomena to the mere power of the imagination. Thus divested of its astrological and cabalistical relations, the doctrine was admitted by Gregory Horstius,* Fabricius Hildanus, James Horstius, and Thomas Bartholine, who have collected cases in its support. James Horstius, in particular, of Helmstadt, who is celebrated as the zealous, but credulous defender of the absurd story of the Silesian golden tooth, against the judicious scepticism of our countryman Duncan Liddel, records several instances in which the body of the infant was marked with the figure of strawberries, cherries, &c. by the force of the imagination of the mother. In short, the greater part of the seventeenth century was remarkable for this imaginative philosophy and its legitimate offspring,—gross credulity in the collection of all sorts of marvellous and prodigious stories. And for this pernicious taste no country in Europe was more conspicuous than Germany; where the natural disposition of the inhabitants to enthusiasm appeared to be favoured by the passion manifested by their scholars for every miraculous event. Not only do we find cases of infants with cats heads, mice-marks, rat-marks, sparrow-marks, mulberry spots, cherries, black-berries, &c., recorded in the writings of some physicians, as Hagendorn, Schook, Schurig, Schrader, Weinrich, Willius, Muller, Steigerthal, Wedelius, Zoutmann, Stalpaart Van der Wiel, &c.; but many such marvellous stories are recited in the EPHEMERIDES NATURAE CURIOSORUM, a work which on that account was not unjustly branded by Cullen as full of old women's stories, the publication of which was sanctioned by a body of physicians. The only exception during the seventeenth century occurs in the writings of RODERICUS A CASTRO, (1628), a native of Lisbon, but professor to the Faculty

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non alias dependentem quam communione universalis alimenti. Diriguntur nempe a desiderio objectum sibi specificanti, non secus atque visus Basilisci vel nutus Torpedinis, reflectum in objectum volitum."—*De Sympatheticis Mediis*, 20, *n. p.* 489.

* De Causis Similitudinis et Dissimilitudinis in Fœtu, Geneva, 4to, 1619.

of Medicine in Pisa. This physician has the good sense to assert that he could not bring himself to believe that imagination only was capable of depriving the conception or embryo of the human shape, and converting it into an animal of that species of which was the animal impressed on the imagination, or at the sight of which the mother had been alarmed; and he goes so far as to say, that in such a case the woman would not be entirely free from the suspicion *bruti alicujus accessus.* *

But one of the strongest instances of confidence in this doctrine occurs in the writings of Sir Kenelm Digby, a man who evidently united to considerable powers of mind, the strongest and most ungovernable taste for the marvellous, and for the operation of secret or occult causes. We allude not merely to his idle story of the influence which the assassination of Rizzio in the chamber of Queen Mary, was said to have exercised on her imagination in modifying the character of James the Sixth, of whom she was then pregnant; but to the general tenour of his writings, and more especially to the opinions delivered and maintained in his 'Treatise of Bodies,' and of 'Man's Soul.' The former story does not require such explanation, and it has been placed in the proper light by Blondel, as we shall see. But in his Treatise on Bodies, which contains much curious observation and not a little good philosophy, if sifted from the chaff of his imaginative opinions, we have considerable discussion on the power of sympathies and antipathies, and among other illustrations the following delectable story. "There was a Lady, a kinswoman of mine, who used much to wear black patches upon her face, as was the fashion among young women, † which I to put her from, used to tell her in jest, that the next child she should go with, whiles the solicitude and care of those patches was so strong in her fantasy, would come into the world with a great black spot on the midst of its forehead; and this apprehension was so lively in the imagination at the time she proved with child, that her daughter was born marked just as the mother had fancied, which there are at hand witnesses enough to confirm, but none more pregnant than the young lady herself, upon whom the mark is yet remaining." ‡

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* "Ex imaginatione solummodo conceptum frustrari forma hominis fierique sensitivum sive animal ejus speciei cujus erat animal imaginatione conceptum, aut ad cujus occursum fœmina conturbata fuerat, *vix aut ne vix quidem adduci possum ut existimem.* Si quippiam simile aliquando, haud libera omnino erit mulier de suspicione bruti alicujus accessus." *Rodericus a Castro de Morbis Mulierum.*

† This fashion, it is to be remembered, prevailed in the reign of James I. and the beginning of that of Charles I.

‡ A Treatise of Bodies by Sir Kenelm Digby, Paris, 1644, p. 329.

We think it cannot be doubted that it was with the view of offering what he conceived to be a more philosophical explanation of these phenomena than had yet appeared, that Malebranche delivered the opinions we have noticed in the beginning of this article. Adopting the coarse physiological theories of the day, regarding the fibres of the brain and their softness and impressibility, and the wild fiction of animal spirits, which were supposed to be nimble and active in early life, but dull, heavy, and immoveable in old age, he mixed with them the gross notions of material impressions, and actual traces of external objects on the incorporeal and intangible essence of mind. Malebranche, it is not difficult to perceive, committed two errors; one in adopting as facts stories which at best were doubtful, and which, if they were true, were to be explained in a different way, and without the agency of the imagination. He threw aside the machinery of the astrological, and cabalistical physiologists, but he substituted others equally inconsistent with the living processes, and with the true nature of intellectual phenomena.

In the hands of Locke, the frost-work of this material system melted away; but he seems to have been unwilling to doubt the powers of imagination entirely. About the same time, 1684, RAYMOND VIEUSSENS in his Neurographia Universalis, not only admitted the doctrine of imaginative influence, but attempted to explain it by referring it to the operation of the nervous system; and about forty years after (1721), RICHARD BRADLEY delivered the same opinion in this country. †

These were the struggles of reason and judgment against the errors of ignorance and the conjectures of fancy. But it was long before a complete triumph could be obtained over prejudices so deeply rooted, and opinions maintained by great authority. Towards the end of the seventeenth century, and beginning of the eighteenth, physicians still believed in the sovereign influence of imagination during pregnancy; and the names of STAHL and FREDERIC HOFFMANN, men certainly not only of powerful genius, but of strong judgment, are still known as the patrons of the theory. The psychology of Stahl is indeed much purer than that of any previous medical author,—his ideas are clearer, and his language more precise and intelligible; but he believes firmly in the power which the intelligent human soul possesses of impressing *alienas configurationes* on the formation of the body, and entertains no doubt of the phantasy of mothers communicating its impressions to the *fœtus in utero.*[*] Nor are the physiological principles of Hoffmann, though differing widely from those of the physician of Halle in other respects, less explicitly in favour of this doctrine. 'Quam nocivæ sunt fœtibus phantasticæ illae gravidarum subitæ ac fortiores impressiones, cum quodam terrore junctae earum in corpore relicta vestigia, maculæ deformationes partium aliunde testantur." [†]

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† A Philosophical Account of the Works of Nature, &c. &c. by Richard Bradley, Fellow of the Royal Society, London. 1721. 4to. p. 168.

In the beginning of the eighteenth century, Boerhaave, who ascribed considerable powers to the imagination, did not scruple to avow his belief in its influence on the fœtus, reasons strongly in its favour, and adduces facts which he regarded as demonstrative of its truth. "With my own eyes have I seen experimental proofs. A lady of rank and great sense happened to walk under a mulberry tree, while one of the berries fell on the tip of her nose; forthwith there was formed on the nose of the embryo a mulberry distinctly marked, in size, colour, with roundish *acini*, rough, and with minute hairs, and that mark became red as often as the tree bore similar ripe mulberries. Here, though nothing took place but a slight touch of the mother's nose, yet a great change was effected in the fœtus." This strange fancy of the annual and periodical change of colour has been very happily ridiculed by one of the authors of the Memoirs of Martinus Scriblerus, probably Dr Arbuthnot, in his usual vein of grave satire, "Being about four years ago in the city of Madrid in quest of natural knowledge, I was informed of a lady who was marked with a pomegranate on the inside of her right thigh, which blossomed and, as it were, seemed to ripen in the due season. Forthwith was I possessed with an insatiable curiosity to view this wonderful phenomenon. I felt the ardour of my passion increase as the season advanced, till, in the month of July, I could no longer contain. I bribed her duenna, was admitted to the bath, saw her undressed, and the wonder displayed."

Van Swieten, the pupil and commentator of Boerhaave, followed his footsteps and imbibed his theory; and Daniel Turner, our earliest native authority on diseases of the skin, was one of its most zealous partisans, and elaborate advocates. He compiled from various sources, ancient and modern, a tolerable, but rather confused account of the doctrine; but the most conspicuous feature in his observations is his boundless credulity, and slavish adherence to the opinions of his authorities.

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[*] "Documentum a posteriori est proprie *efficacia* illa hujusmodi maternarum impressionum in *ipsum animum etiam* infantis; unde vel immoderata desideria vel insuperabiles terrores, metus, anxietates ex eodem tali matris *phantasmate*, quale quidem alias, imo aliquando simul conformationem ejusmodi heteroclitam corpori ipsi una imprimit." "Nimio notior est energia imaginationis quæ non solum partes aliquas auferre potest, etiam eo tempore quo jam penitus formatæ fuerant, sed etiam non solum novas sed penitus alienas suscitare." p. 276, de Spermate. Georgii Ernesti Stahlii Mem. Medica Vera, &c., 1708 and 1777, p. 373.

[†] Frederici Hoffmanni Opera Om. tom. i. Philosophiæ Corporis Humani vivi et sani liber primus, sectio iii. cap. 6. § xiv. p. 97.

The time however was fast approaching, at which the truth of this extravagant theory was to be put to the test of examination and sound judgment. More correct ideas on the nature, not only of the human mind, but on its operation alleged or established on the material frame; and, above all, physiologists began to understand more distinctly the nature of the processes of the animal body, and the means by which they were effected. The powers of the immaterial mind were beginning to be more justly appreciated; and the sovereign authority of the imagination was at length called in question. The first blow which was struck at the theory was by M. Marcot of the Royal Society of Medicine at Montpellier in the year 1716, on occasion of the case of a monstrous infant, the history of which he communicated to the Society.* On the forehead of this infant was a soft, spongy, livid mass, which, to the ignorant eyes of the midwife, bore the resemblance of a toad, and which was, therefore, without ceremony, marked as a monstrous production. The child died in about twelve or fifteen hours, and was found, on dissection, to be an acephalous fœtus, destitute of brain and spinal chord, and having on the forehead a soft, parenchymatous, vascular mass, presenting bodies like hydatids, which being of different sizes, made it seem irregular on the surface, and gave it the appearance of a *toad*. M. Marcot ascribes every deviation in the shape of the fœtus, whether by addition, defect, or confusion of parts, to obstruction, compression, or twisting of the umbilical chord, and whatever may interrupt or modify the passage of blood to it. He cannot believe that the imagination of the mother is adequate to produce changes so considerable; he denies the intimate connexion of the fœtus with the mother;—that the infant sees and hears whatever is seen and heard by the mother; and though he admits the communication of diseases of morbid disposition, and the hereditary transmission of temperament, appetite and inclinations, yet these, he justly remarks, would prove nothing regarding the power of imagination, and are quite independent of it. He refutes, at some length, the errors of Malebranche, especially regarding the alleged sympathetic or constitutional terror of James First at the sight of a sword; and while he does not deny entirely the facts, he contends that the resemblances are rare; and that it would be found that the idea of the mother having seen or felt such object as that from which the resemblance was supposed to be derived, had occurred subsequent to the birth of the infant, and had been eagerly laid hold of to explain the deformity. Thus, a woman who finds that she is so unfortunate as to give birth to a child with a cat's head, reflects within herself on the cause of a phenomenon so extraordinary; she imagines she has been frightened by the sight of this animal; she says so; and her friends fail not to ascribe the cause of her misfortune to the horror with which she had been seized. In like manner, if a woman gives birth to an infant which bears the mark of any fruit, she is interrogated if she had not the desire of eating this fruit during her pregnancy, if she has not been refused it, and if she has not scratched some part of her person under the influence of the disappointment; for all these circumstances are necessary to the success of the fœtal phenomenon. She understands what is meant, and generally answers by some story which suits the purpose of the querists, but is rarely founded in truth. Hence the false prejudices, according to M. Marcot, which pass for assured maxims and established truths. Hence those questions about the operation of the imagination of the mother on the body of the infant, which it is impossible to resolve, insomuch as they are founded on a false supposition.

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* Histoire et Memoires de l'Academie Royale des Sciences, Année 1716. A Amsterdam 1719.— Memoires, p. 415.

M. Marcot, however, admits that there are examples of birth-spots on the persons of children whose mothers were not only unconscious of their nature and supposed origin, but were not aware of any particular longing during the period of pregnancy. He was in this condition himself, having a reddish mark on the right hand, which could be referred to no sort of fruit or food under the sun, and which his mother could never explain.

Not long after, the credulity displayed by Dr Turner in the work already mentioned, roused a vigorous assailant of the imaginative theory, in the person of DR JAMES BLONDEL. At this time, 1727, so little were the opinions of Marcot, just mentioned, known or relished, and so general was the belief in favour of the old system, that Blondel, though he writes in a bold and decided tone, is anxious to apologize for combating a vulgar error, which had prevailed, he says, for several ages, though inconsistent with experience, right reason, and anatomical knowledge. His principal arguments against the theory are the following. *First*, The imagination is observed to be very powerful in some pregnant women, without producing any effect on the fœtus. * *Secondly*, Infants are marked with variously figured spots, or may be very much deformed, where no appetite, longing, or operation of the fancy of the mother, ever took place or could be recognised. *Thirdly*, Many of the deformities and defects which are falsely and unreasonably ascribed to the operation of the fancy, depend upon other causes, generally natural, during the course of pregnancy, or accidental, previous to, or at the period of delivery, or, in short, are fortuitous, and not in all cases to be explained. In other instances, the facts are erroneous; and the credulity of witnesses, the love of imposture, or the taste for wanton falsehoods, are all to be regarded as contributing to the propagation of error. These positions he establishes by much sound and forcible reasoning, in the course of which he shows the absurdity of many of the current stories. Among others, he remarks on the first given by Malebranche, that it was singular that, as the imagination of the mother was so expeditious and perfect in conveying to the person of the infant each of the blows of the executioner, it did not convey that also which is inflicted on the stomach, vulgarly called the *coup de grace*. On the second, he justly observes, that the resemblance which forms its subject is greatly too remote, and that the whole story savours of the bigotry and ignorance of extravagant enthusiasm. In an elaborate Supplement, he reviews, with great severity, the cases and arguments which the credulity of Dr Turner had allowed him to adduce.

Blondel wrote previous to the publication of Boerhaave's Institutions of Medicine, and his work was, therefore, known to the Dutch professor. It made no other impression on him, however, than merely to confirm him, if possible, most strongly in his original opinion. "Novi," says he, with great coolness, "quid *Anglus* scripta ediderit, sarcastico stylo in hanc vim formatricem invectus. Nihil inde sequitur, quod phenomeni causam non intelligamus." And he proceeds to relate various cases in support of his opinion. Blondel's attack produced a defence from Turner, but without the adduction of any very conclusive arguments. The inclinations of the profession, however, appear to have still been in favour of the imaginative theory; and in 1740, a paper by Daniel de Superville, chief physician to the Margrave of Brandenburgh-Bareuth, appeared in the Transactions of the Royal Society of London, in the conclusion of which it is stoutly maintained. De Superville asserts, that he had seen in a sow, just slaughtered, seven pigs, all of which had the bloody mark of the knife about their necks; and he mentions the case of a cloth-shearer in Holland, who, falling into the hands of some drunken young fellows, was stabbed with more than twenty wounds. "He was to be married that very week; his sweetheart saw his corpse naked with all those wounds, and was two days after delivered of a dead child, which had the marks of the wounds in the same places of its body where the mother had observed them on her dead lover." He contends, that the fœtus is part of the mother, insomuch as it derives its nutriment from the secundines, which are closely united to the womb; and argues, that the incomprehensibility of the fact is no argument against its existence. * It can scarcely be doubted that De Superville displays more credulity than judgment.

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* "As a lady in London, with a violent antipathy to cats, was one evening going to bed, a huge cat sprung suddenly from under the bed on her furiously, and was with difficulty beaten off. The poor lady was so much frightened, that she fainted; but nevertheless, notwithstanding this shocking surprise, she was safely delivered, three months after, of a very pretty girl, who had neither whiskers nor claws, nor any resemblance to a cat; and, what is still more remarkable, this child amused herself in playing with cats, and very often in tormenting them, as if she wished to avenge the affront which her mother had received."—*Blondel.*

A new and more powerful opponent, however, appeared in the eloquent and philosophical Buffon, who is entitled to the merit of first generalizing the researches of physiologists on the function of generation. He justly remarked, that the resemblance of birth-spots to any other object, is more frequently dependent on the fancy of those who see them than on that of the mother; that the alleged periodical changes of colour were much more frequent than was vulgarly supposed; and instead of depending on the occult sympathy between them and the objects which they were supposed to resemble, took place whenever the motion of the blood was accelerated, which is most steady and permanent during the heat of summer. These spots are yellow, or red, or black, according as the blood communicates these tints to the skin, when it enters the vessels in too great quantity. It may be further remarked, that the exceedingly vascular nature of most *nævi materni*, covered as they are by a mere pellicle of skin, is very favourable to these periodical changes, according to the revolutions of the season, and the consequent temperature of the atmosphere.

With regard to the effects of any principal emotion, he remarks, that as our sensations do not resemble the objects which cause them, it is impossible that desire, terror, horror, or any passion or internal emotion can produce real representations of these objects; and it is as easy to suppose that the fancy of a fowl, before which the neck of a cock has been twisted, will produce from the eggs on which she is sitting, chickens with twisted necks, as it is to believe the story of the woman who saw the man broken on the wheel. Such shocks might be followed by the death of the fœtus, but not by the exact imitation which authors have maintained. But even admitting that a child was produced with fractures in the same places in which the limbs of felons are broken, it does not follow that this phenomenon arose from the spectacle which the mother witnessed, or from the influence of her imagination on the infant. Such effects might, if carefully examined, be much more satisfactorily traced to the operation of causes the nature of which is much more distinctly understood. Syphilis, rickets, and scrofula may produce effects which, to inexperienced observers, would appear like distinct fractures.

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* Philosophical Transactions, giving some Account of the present Undertakings, Studies, and Labours of the Ingenious in many considerable Parts of the World. Vol. XLI. Part I. for the Years 1739 and 1740. London, 1744.

The force of these arguments has been generally supposed to be sufficiently strong to annihilate the imaginative theory entirely. They were corroborated by the anonymous writer of *Lettres sur l'Imagination des Femmes Enceintes*, in 1746, by the observations of M. ELLER, in 1756, in the Transactions of the Berlin Academy, * by Krause and Roederer in express dissertations published the same year at St Petersburgh, and by HALLER in his great work on Physiology, in 1766. As this last eminent physiologist examined the arguments on both sides with his usual acuteness and deliberation, it may not be improper to notice them shortly.

When Haller published the physiological lectures of his preceptor Boerhaave in 1758, † he does not appear to have made up his mind on the merits of the imaginative theory. " Hæc nævorum historia mihi difficillima videtur, " he says. It was evident that he hesitated on the one hand between the great authority of his revered Boerhaave, and the dictates of sound sense and the reasonings of Blondel, who had written some twenty years before, on the other. He had also examined with great care all the known histories recorded in authors, and has left a most remarkable monument of his research and diligence in his notes on Boerhaave. The apparent good faith of his authorities operated forcibly on his belief; but the absurd and incredible nature of the stories, and the impossibility of explaining them on any rational principle, staggered his judgment. Neither the *anima structrix* of Stahl, nor *the communication of spirits and nerves* proposed by Vieussens and Bradley, was adequate in his judgment to account for the phenomena. "Meanwhile," says he, "we are overwhelmed with innumerable stories of deformities of every description which have affected the fœtus by the fault of the mother, more frequently indeed in the female of the human subject, but sometimes in those of brute animals." Without denying them altogether, he admitted that he had seen moles, spots, and excrescences, which, by a lively imagination, might be transformed into mulberries, cherries, mice, sparrows, flames, &c., which might have arisen from any cause capable of eroding or injuring the skin. He agreed however with Blondel in admitting, that a very great number of deformities or monstrosities take place without any affection of the mind, or any disorder of the parent; and that there were no grounds for charging the incomprehensible communication between the mother and the fœtus with the formation of all these trifling defects. The case of Malebranche, and others in the Ephemerides Naturae Curiosorum, he regarded as false or erroneously reported.

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* Histoire de l'Academie Royale des Sciences et Belles Lettres, Anno, 1756. A Berlin 1758. Tom. 12, p. 1.

† Note on Boerhaave de Conceptu, p. 330, Vol. V. p. 2.

These arguments were repeated, expanded, and enforced in his Elements of Physiology. But it is chiefly in his reasonings on the origin of *Nævi* which was regarded as the principal argument, that much interest is found. He did not deny that the bloodvessels of the mother were connected with the bloodvessels of the fœtus. But the admission of this would never explain why the inordinate motion of the blood in the part of the body of the fœtus corresponding to that which the mother touched in her own person, should leave its trace there rather than in any other, and especially rather than in a more tender part of the fœtus. Against the opinion advanced by VIEUSSENS and BRADLEY, and afterwards repeated by Turner, [*] that impressions of this nature are conveyed by the nervous system, he stated two very valid objections. First, the shape of a mulberry, or any other object, cannot reach the brain equal to the mulberry seen or longed for; but something infinitely less, whether it is moved or otherwise applied. Secondly, if transmission of any impression takes place, it must be through the medium of nerves, the only channel by which the impulses of the immaterial soul are conveyed. *But no nerves pass from the mother to the fœtus, as is very well known;* nor could these nerves elude inspection, when they must proceed in a body from the placenta to the cord, though a few may come from the liver to the ligaments and navel. [†]

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[*] A Treatise of Diseases incident to the Skin, by Daniel Turner, &c. London, 1776, p. 160.

[†] Elementa Physiologiæ, Tom. VIII. Lib. XXIX. Sect. II. § XXI. p. 129.

These arguments have generally been thought satisfactory; and the belief in the influence of the imagination on the *foetus in utero*, has been tacitly, if not gradually and entirely relinquished since the time of Haller. It appeared to be the general sentiment of physiologists that nothing remained for further inquiry after the question had been examined with so much deliberation and judgment, as the Swiss physiologist bestowed on it; and it was therefore looked on as decided, so far at least as facts supplied, and as the nature of the subject admitted. Sixty years have elapsed since this opinion was delivered, and no fact or objection of any material consequence has been adduced to refute or invalidate the grounds on which it was formed. At length, however, Sir Everard Home has, after much research and regularly planned inquiry, according to his own account, succeeded in making a discovery, which, if established, may certainly invalidate the conclusions of Haller. Without appearing to be aware of the opinion of Vieussens and Bradley on the supposed power of the nerves in accounting for the effects of the imaginative theory, or the argument of Haller deduced from their non-existence between the mother and fœtus, or indeed with any part of the history of his subject, the learned Baronet has proved the existence of *nerves in the placenta and cord*, and has thus deprived the argument of Haller of one of its most powerful sinews. We know not that this discovery, though it were well established, will make much difference on the total and ultimate merits of the question. We would not be understood to favour such an opinion; we speak purely in reference to the argument of Haller.

The nature of minute researches, either on the structure or functions of the nervous system, is so peculiar, that our readers will undoubtedly prefer seeing the description in the language of the author.

" This I have now been enabled to do in no common degree, by discovering nerves in both the fœtal and maternal portions of the placenta. This discovery, I am proud to say, was not the result of accident, but of a regularly arranged plan for that purpose.

" In examining, at my desire, the structure of the horns of the fallow-deer, during their growth, while covered with velvet, Mr Bauer found them abundantly supplied with nerves. The circumstance of nerves being met with, where nerves are not only unnecessary, but even where the parts are unfitted for their office till the nerves are removed, which takes place as soon as the horns are full grown, makes it appear that nerves answer some other purposes in the animal economy, besides regulating the actions of arteries; an office which, many years since, I not only considered them to perform, but illustrated my opinion by the effects of irritation of the *par vagum* and great sympathetic nerve on the carotid artery. Since that time, by considering the incubation of the chick, I have been led to believe, that the arteries are indebted to the nerves for their formation; and so strong was the conviction on my mind of this being the case, that even the circumstance of the placenta, whose bloodvessels are very numerous, having been suspected to have no nerves, did not induce me to abandon it; since until it is proved that the placenta is void of nerves, there is no argument against me. This was a point of all others, that no one could determine so well as Mr Bauer. I therefore most earnestly requested him to employ his microscopical observations on this subject, and supplied him with the placenta of a seal, in which the arteries and veins had been injected; and as in that animal the umbilical vessels are not twisted, the nerves will be more exposed, and the parts having been in spirit, will have lost that transparency belonging to them in a recent state, by which they are less readily distinguished from bloodvessels.

"In this specimen Mr Bauer has shown, that nerves are not only conspicuous surrounding the umbilical arteries, but has demonstrated them in the portion belonging to the uterus. For the appearance they put on, I must refer to the annexed drawing.

"I may here remark, that in no communication which I have made to the Society, assisted by Mr Bauer's labours, has any appearance been mentioned or represented that I have not myself distinctly seen; for although I am not equal to the nice adjustment of the microscope, which indeed appears peculiar to Mr Bauer, yet, when adjusted by him, the appearances before they were described had been rendered visible to me.

"In looking at objects so much magnified beyond what they appear to the naked eye, it will not be unnatural for many of the members to ask, how I am sure that these are really nerves, and not the secondary order of blood-vessels, too small to carry red blood, and therefore, when their contents have been coagulated, appear to be cords? My answer to this question is, to recommend an inspection of the drawings, in which it will be seen, that these are not continuations of other branches, but form a trellis-work upon the arterial trunks, in a manner totally different from any thing met with either in the ramifications of arteries or veins; and when they are dried upon glass, they reflect the light with a degree of splendour like the human hairs when these are quite white. When the nerves are very minutely examined, each fibre appears to consist of a row of small globules connected with one another.

"At the time the nerves in the placenta were discovered, Sir Stamford Raffles, whose misfortune in having lost the most valuable collection in Natural History ever made in the East Indies, by the ship taking fire, every one must feel for, brought me from Sumatra the pregnant uterus of the Tapir of that country; and as in that animal the umbilical cord is connected with the chorion (there being no placenta), I examined the transparent portion of the chorion, along which the branches of the *funis* pass, before they arrive at the spongy part, and there the nerves are so conspicuous, that Mr Bauer's representation of them of the natural size is annexed.

"The principal object of the present Lecture is to establish the fact of nerves existing in the placenta,—and in those animals in which there is no placenta, in the flocculent chorion which is substituted for it; and it is a curious fact that they should be largest in the latter.

"This discovery places the placentular circulation in a new point of view, since, from the known influence of the nerves on blood-vessels, it is reasonable to believe that, during life, there are branches of communication between those of the uterus and fœtus, although too minute to be explored in the dead body. The erection of the penis cannot be produced after death by injecting the arteries, although, when the nerves are excited, the smaller branches give a a ready passage to the blood."

At this time Sir Everard home appears to have satisfied himself of the existence of nervous filaments in the placenta of the lower animals only. He informs us, however, in a postscript, that he and Mr Bauer, unknown to each other, succeeded at the same time in detecting nerves in the human navel-string and placenta, in a preparation belonging to Mr Brookes, which had been successfully injected and preserved in spirits for forty years. As the existence of nerves in the placenta forms an important article in the argument of Haller, it becomes a question whether it will impair or destroy the strength of this argument. From the conclusions which Sir E. Home draws, it is manifest that he regards the placental nerves as competent to explain the whole train of those phenomena in the fœtus, which have either been traced to the corporeal constitution, or the mental emotions of the mother. The learned Baronet, however, has impaired the force of his arguments by proving too much, and has manifestly shown the weakness of his cause, by having recourse to facts in the explanation of which no difficulty occurred, and in which the agency of nerves was certainly not absolutely necessary. Sir Everard shall speak in his own person.

"Now that it is known by the discovery of nerves in the placenta, that the brain of the child, as well as every part of its body is connected by the medium of nerves with the brain of the mother, we are led to understand the degree of dependence in which the fœtus is kept during the whole time of utero-gestation.

"The smallpox being in some instances communicated from the mother to the child, which has until now been considered as an extraordinary fact, and not to be accounted for, is readily explained, since absorption depends fully as much upon nervous influence as the action of the arteries; a child in utero having an ague, is in itself almost a proof of the placenta having nerves.

"A child being born without a brain is not to be marvelled at, the nerves of the child being connected with the brain of the mother."

Now, it is impossible to imagine a more finished specimen of inconclusive reasoning and imperfect physiology than what is contained in these paragraphs. It is manifest, that whenever the communication of a disease, or of a form of morbid action, depends on a tangible or material morbid agent, it is quite unnecessary to have recourse to refined and fanciful means, the existence of which is not only uncertain, but the operation of which is exceedingly doubtful, and at best incapable of being proved. The transmission of smallpox, of the venereal disease, nay of ague, from the mother to the embryo or infant in her womb, may be easily understood without having recourse to the supposition of the agency of the nervous system. In the two first cases, there is a material morbid poison circulating in the blood, and liable to be deposited in the tissues; and if a similar poison is not acting in the third case, we know that the phenomena of ague depend upon some material agent, which affects chiefly the circulation. The acephalous fœtus is certainly not to be ascribed to any communication between the nervous system of the mother and of the fœtus.

Nor is the following conclusion more pertinent.

"The immediate division of the navel-string at the moment of the birth, in some particular instances having hazarded the life of the child, hinted at by Dr Denman, shows the accuracy of his observations."

This may show the accuracy of Dr Denman's observations, as it would do of any sensible man who knows that premature division of the navel string produces death by attempting respiratory life, before the lungs of the infant are fitted for it. But we cannot perceive in what manner it illustrates the connexion between the nervous system of the mother and of the fœtus.

But by far the most brilliant application of this discovery is yet to come in the revival, or if you will, corroboration of the doctrine of Vieussens and Bradley.

"Till this discovery was made" says Sir Everard Home, "we had no mode of estimating the influence that could be produced upon the child, by the affections of the mind or the body of the mother; and therefore, the instances that have occurred were considered as idle stories or accidental occurrences for which no satisfactory reason could be assigned; and this upon no better ground than that they do not always take place under similar circumstances, which nothing connected with nerves ever does.

"That they do sometimes occur, no one can be so hardy as to deny; and when they do, we cannot now be at a loss for a mode of accounting for their doing so."

In confirmation of this sagacious opinion, the author proceeds to relate several cases, which are very similar to all other cases of the same nature, scattered in the writings of physicians, and need not therefore be repeated. It is sufficient to say, that the first, which is the instance of the mare, which, having a foal by a quagga, had three quagga-marked foals in succession by a Persian horse, revives the doctrines already noticed of OPPIAN and BERENGER. The second is an instance of harelip occurring in an infant whose mother had, while pregnant, been robbed near Woolwich by an artillery-man with that deformity. The third is one of a lady of unusually nervous habit, who, during pregnancy, had been alarmed by a huge Newfoundland dog springing with his two fore-paws on her sides; "when the child was born, there was a claret-coloured mark upon the two parts of the belly corresponding with the places on which the paws of the dog seized."

The fourth case must be perused as nearly as possible in the words of the author.

"An Italian woman, twenty-three years of age, when, by her reckoning, three months and three weeks gone with her third child, was travelling in a caravan with the baggage of the Duke of Wellington's army. In the middle of the night, in a violent storm, while she was fast asleep, a small monkey, with a long chain, upon the roof of the caravan, took refuge in it, crept under her loins, and fell asleep; she awoke, feeling uneasy from the pressure of the monkey, and put her hand down to scratch the part, but came upon the monkey's head, by which it awoke and bit her fingers, and in its alarm got fast hold of her loins. The woman went into fits, and was some minutes before she recovered herself; it was suspected she would miscarry, but she went her full time. When the child was born, it measured between seven and eight inches in length, and weighed one pound. This was in France. The child was reared with great difficulty. It was carried afterwards to Ireland, where it was afflicted with a hacking cough. It was brought to England, in expectation of the cough being relieved, but died soon after, just before it completed its ninth year. I saw it before its death, when it was much emaciated, and measured exactly twenty-two inches. It could walk alone, but not more confident in itself, or firm on its legs, than a healthy infant of sixteen months. Its sight was very quick, particularly in seeing bright objects; it was delighted with every thing showy, much pleased with ornaments in its own dress, could speak in a very low tone and shrill voice, and had some taste for music, but had few English words; it also appeared very sensible of kindness, and remembered perfectly those from whom it had received attention. After death it was found that the fontanelle of the head had closed; no fat was any where met with but at the bottom of the orbits. The uterus was as small as in a fœtus between three and four months, not being at all developed, whereas in a newborn child it has acquired a considerable size. It was closely attached to the posterior surface of the urinary bladder, apparently by inflammation. The bladder was distended with urine, and the size of a turkey's egg. As the child had never passed its water freely from the time of its birth, *this affection of the bladder must have taken place at the same time with the injury produced by the monkey's gripe on the loins of the mother.*

"I examined an abortion, which was considered by Mr Clarke, Teacher in Midwifery, to be about three months and a half after impregnation; and on comparing the ovaria with those of the dwarf, they were nearly of the same size, but not quite so long; the difference, however, was scarcely observable.

"After death, it was found that the mother has had a fifth child in Ireland, which, like her former children, was of the common size when born, and has nothing particular in its appearance."

It would require great stretch of credulity indeed, and much faith in the powers of fancy through the medium of the placental nerves, to admit this story to be a legitimate example of their influence. That the infant was an instance of monstrosity, that is, of deviation from the ordinary form and proportions of the human body, may be readily admitted; and it may be further granted, that this deviation in the person of the infant was occasioned by the corporeal injury and mental shock which the poor woman sustained from the natural but rude attack of the monkey; but that the fancy of the mother, operating on the nerves proceeding from the placenta and cord to the fœtus, was the channel through which the mischief was effected, is an inference which neither flows from the phenomena, nor could, without the grossest assumption, be satisfactorily made to correspond with the known facts of utero-gestation and the growth of the fœtus.

In two other cases which were stated to him, the author says he has the most implicit confidence.

"In one, a lady in early pregnancy was frightened by a sailor with one arm; and her child was born under the same deformity. In another, this occurred late in pregnancy. No effect was produced in the infant then in the womb; but in the next child, though every alarm in her mind had subsided, the deformity was found to have taken place."

Of such marvellous stories it would be extremely idle to attempt either a lengthened examination or a serious refutation. It is enough to say, that not only are the facts too doubtful, but the alleged instances are too few to warrant conclusions such as are delivered in the paper before us. That instances of infants born with hare-lip, with one arm, with marks and various other deformities or imperfections, frequently occur, is well known, and can never be questioned; but where is the instance in which the circumstance of any object presented to he senses, or impressing the imagination of the mother, couldbe indubitably shown to be connected with these facts, much less necessarily connected as cause and effect? It is a most true and invaluable observation, that there are in medicine more false facts than false theories; for however easy it appears to be to observe and record facts properly, it is the most difficult task of the sound and unprejudiced physician; and there are some facts, in physiology particularly, which few indeed are competent to observe. An appetite for extraordinary stories and marvellous tales is often mistaken for the philosophical spirit of collecting facts; and the indiscriminate love of collecting cases is supposed to be the most meritorious duty of the student of nature. Such labours we would neither undervalue nor discourage; but it ought never to be forgotten, that, to be useful, they must be under the control of sound judgment, and a most thorough knowledge of all the fallacies to which the observation of facts so varied and so complicated as those in physiological inquiries are, is exposed. We can scarcely conclude better than in the words of M. Marcot. "All recorded facts are not true, and the results of experience are not always worthy of trust. Many things are true which pass for false; and many false are admitted as true. Men do not always observe with cool and unbiassed senses; they think they see more than they do indeed, *Plus vident quam quod vident*. They build a system on false principles; they fall into error, and lead others along with them. They speak often of things which they have not seen, as if they had seen them; and indolence, indifference, or the love of imitation, prevents others from ascertaining the truth."

III.

Sketches of the most prevalent Diseases of India; comprising a Treatise on the Epidemic Cholera of the East; Statistical and Topographical Reports of the Diseases in the different Divisions of the Army under the Madras Presidency; embracing also the Annual Rate of Mortality, &c. of European Troops; and Practical Observations on the Effects of Calomel on the Alimentary Canal, and on the Diseases most prevalent in India. By JAMES ANNESLEY, Esq., Madras Medical Establishment. Underwood, London, 1825. 8vo. pp. 464.

THIS work, one of the most interesting which has for some time been contributed to medical science by the practitioners of the East, is the result of twenty-five years' experience in the observation and treatment of its diseases in every variety of circumstances; and more particularly of five years' superintendence of the General Hospital at Madras, during the ravages committed throughout Hindostan by the Epidemic Cholera. It includes, as will be perceived from the title, a great variety of topics, most of them too extensive and too complicated to admit of complete discussion in a single octavo volume, and therefore quite unfit for investigation in a succinct review. Of the remainder we shall single out for the present those which are most interesting from their novelty and from the light which it has been the author's object to throw by them over one of the most baneful and embarrassing maladies of our Eastern dominions. We shall confine our attention chiefly to his views regarding the nature and treatment of Epidemic Cholera, and the effects and mode of action of calomel in that and some other analogous disorders. To these views we shall add such strictures and illustrations as have occurred to us during the perusal of the work itself, and from an examination of the voluminous and valuable Reports on Cholera published at Madras in 1824 by the Medical Board of that Presidency.* The other topics regarding Cholera, which are embraced by the Madras Reports, and the Reports of the two other Presidencies, and which have likewise received a share of Mr Annesley's attention,—more particularly the question respecting its cause and mode of propagation,—may be made the subject of separate scrutiny at another opportunity.

It would appear, on collecting and combining the numerous authentic documents which have at length been procured on the subject, that the Epidemic Cholera has not caused such extensive devastation as general rumour and local experience had led most people to believe. But nevertheless it still remains true, that few diseases have been more prevalent of late years in the Company's territories, and that none of the prevalent diseases is more dangerous, or carries off such a large proportion of those whom it attacks. According to the documents collected by the Madras Medical Board, the number of deaths caused by it in the army of that Presidency, during 1818 and the four subsequent years, has been 4430, of which 695 occurred among the European troops, and 3735 among the Sepoys. The number attacked was 19,494, namely, 3664 Europeans, and 15,830 Natives. The average strength of the army, during the period included by the Reports, being 10,112 Europeans, and 73,254 Natives, it follows, that in five years 23⅓ per cent. of the troops were attacked, and that of those attacked, 22¾ per cent. were carried off, or 5⅓ per cent. of the whole force of the army. The epidemic cholera therefore is confessedly one of the most fearful scourges of our Indian Empire, and must be an object of paramount interest to its government, to its medical officers, and to the medical profession generally. The Indian governments have fulfilled their duty most signally by the pains they have taken to present the public with authentic documents, which in extent and minuteness are almost unprecedented in the history of medicine. The Company's medical officers seem sufficiently disposed to imitate the example set them by their employers. But it is rather singular, that the profession at home, for the sake of whose opinions and researches the official reports have in part been published, have hitherto paid so little regard to what is so reasonably expected from them.

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* Report of the Epidemic Cholera as it has appeared in the territories subject to the Presidency of Fort St George. Drawn up by order of the Government, under the superintendence of the Medical Board, by W. Scott, Surgeon and Secretary to the Board. *See* our Number for July 1825.

We cannot find a better method of giving support to this part of the Company's objects, than by drawing the attention of our readers to the work of Mr Annesley. It contains all the general facts and most interesting local occurrences which have been laboriously recorded in the Reports; but presents them to the general reader in a more popular and attractive form; while at the same time the addition of several copious and circumstantial tables will enable those more deeply interested in the subject to study it with the greatest minuteness. We may add, what is no trifling recommendation, that his book is written in a style of such simplicity and chasteness as we very rarely meet with in medical writings of the present day. Its distribution is most methodical. After a few remarks on the question, whether the cholera, *as it appeared epidemically*, is a new disease, or one formerly known to Indian practitioners, he proceeds to treat first of its symptoms, then of its pathology, next of its causes, and lastly of its treatment. This concludes the First Part of the work. The Second Part consists of thirty-one Tables of Reports on the diseases of the Army under the Madras Presidency, including of course tabular views of the progress and prevalence of Cholera. The Third and last Part is an inquiry, in the way of observation in man and experiment on brutes, into the effects of calomel on the alimentary canal, and generally on the diseases of India. We shall confine our remarks, as already announced, to his inquiries into the pathology and proximate cause of cholera, and its treatment especially as regards two remedies—blood-letting and calomel. For a detailed account of the symptoms, the reader is referred to an account of the Bombay Report, in the 16th and 24th volumes of this Journal.

We shall take no notice at present of our author's preliminary remarks on the question of the novelty of the disease, farther than by observing, that he believes it is a disease of new occurrence, whose features are certainly never assumed by the cholera of Europe, and, so far as can be ascertained, as little by the sporadic cholera of the East. In this opinion, however, it is conceived he is not borne out by the investigations of the Madras Medical Board. For, as we may afterwards have occasion to show, various proofs have been procured by them of its pre-existence, though probably never in such a violent form, or over such an extent of country.

But whatever opinion be formed of its novelty, there does not seem the slightest reason to doubt that the name of cholera is completely misapplied. The most characteristic symptom of cholera, the discharge of bile from the stomach and intestines, is completely wanting in the disease we are now considering. Not only is there no increased flow of the bile; but its discharge from the gall-bladder is prevented entirely, nay, in all likelihood its secretion is arrested altogether. Another leading symptom of cholera has likewise been absent in the late epidemic of Hindostan, namely tormina. The pain of the bowels, which was present in the generality of instances, was of a totally different description, and is represented by Mr Annesley as consisting for the most part of a burning sensation seated between the scrobiculus cordis and the navel.

The actual symptoms of the disease as they are laid down by Mr Annesley, by the Madras Reports, and by the Reports of the two other Presidencies, from which we gave copious extracts in our Number for July 1820, may be referred to three heads, those of derangement of the alimentary canal, of the muscular system, and of the circulation. By considering each of these classes of symptoms separately as they appeared in the different stages of the disease, and by uniting them with the corresponding morbid appearances produced by it, we shall be better able to appreciate the views of our author regarding its nature.

The affection of the alimentary canal is very generally, viewed by East India practitioners as of a peculiar kind. It consists of complete interruption of the process of digestion, of the stoppage of all the natural secretions connected with that process, and of the exhalation of a fluid different from that thrown out from the mucous membrane during health, or probably in any other disorder. The substance thrown out is variously described by different observers as a mere serous fluid, a fluid like thin rice-water, or a mixture of milk and water, or cream, occasionally containing a large quantity of white shreds or flakes, more rarely brownish or tinged with blood. This affection of the alimentary canal generally existed from a very early period. One of the first symptoms of the invasion of the disease was a sudden call to stool, and the discharge of the natural fæces was soon followed by that of the fluid mentioned above.

The morbid appearances of the alimentary canal connected with the foregoing symptoms, have been very carefully investigated by Mr Annesley. His researches, indeed, appear to be the only ones, included in the Madras Reports, which have been made with minuteness and accuracy. The *stomach* was generally somewhat congested with venous blood outwardly, and still more so inwardly. The chief congestion was in the mucous coat, which was likewise occasionally corrugated, thickened, and doughy; and the whole coats of the organ were sometimes softer than natural. In a few instances the inner membrane of the stomach was of a livelier red colour. We may add, too, a circumstance omitted by Mr Annesley, but clearly ascertained by the Reports of the different Presidencies, that the stomach was occasionally quite natural in appearance. It almost always contained more or less of a watery or muddy fluid, greenish, yellowish, brownish, or black in colour. The *small intestines* externally had a doughy, thickened appearance, their veins were turgid, and the general colour of their surface varied from a pale vermilion to a dark purple. In the early stage of the disease the internal coat of the small intestines was pale in the upper portion, dark and congested in the lower; but when the patient lived till a more advanced stage, the colour was brighter. In its early stage the villous coat was clogged with a viscid, thick, clayey substance, which in the more advanced stage was detached more or less, and floated in shreds and flocculi in a fluid such as that discharged by stool. "The quantity of this earthey-looking stuff," adds Mr Jameson in the Bengal Report, "was sometimes so large as to plaster the whole villous coat, and to leave a thick sediment in passing through the sheet in which the corpse was wrapped. p. 72. The *great intestines*, like the lesser, were generally congested both within and without, and filled with a slime or fluid like that just described. In some of the very rapid cases there was no morbid appearance in the whole alimentary canal, except the effusion of the peculiar muddy fluid. (*Bengal Report*, 72.)

No appearance of much note was ever found in the other abdominal viscera. The liver was sound, except that it was sometimes gorged with venous blood; the gall-bladder contained more or less bile, generally thickened, and greener than natural, and the mouth of its duct was sometimes constricted; the spleen was generally turgid; the kidneys healthy; the urinary bladder excessively contracted and empty.

The foregoing abstract of the symptoms and morbid appearances referrible to the alimentary canal shows that it was affected throughout with venous congestion; and that a matter of an unknown, or at least of an undetermined nature, was thrown out from its surface. We shall now take a short view of the different opinions formed by the practitioners of the East regarding the nature of the affection thus constituted.

The first view taken by very many of those who have contributed to the Reports, and evidently the opinion to which some of Mr Annesley's statements tend, although he does not distinctly express it any where, is, that the affection is of an inflammatory nature. To this opinion its supporters have been naturally led by the symptom of burning pain in the abdomen, by the appearance of vascular turgescence after death, and by the effects to be mentioned hereafter of blood-letting and calomel. None of these grounds, however, are sufficiently substantial. As to the symptom of burning pain, that is by no means diagnostic of inflammation. With regard to the appearance of vascular congestion in the stomach and intestines, it may be objected, that it never amounted to any thing else than *venous* congestion; which is not adequate to constitute, although it may ultimately bring on inflammation. It is mentioned, indeed, that in some instances the vascularity was florid, nay that in a few cases coagulable lymph with subjacent arterial vascularity was formed on the mucous coat of the stomach and intestines. (*Madras Report*, 177.) But these appearances were rare, and occurred either in such cases only as had been marked with violent reaction from the beginning,—a state quite opposite to what generally occurred—or in the advanced stage of cases that had passed successfully through the previous stage of collapse. Lastly, in respect to the operation of remedies it may be argued, that, in the absence of better proof, this is a very fallacious kind of evidence, and that in the present instance it admits, as will be shown hereafter, of a totally different explanation. We are inclined, therefore, to agree with Mr Jameson and Mr Scott in thinking, that the evidence of its having been an inflammatory affection is quite unsatisfactory. At the same time it is highly probable, that it was accompanied with a certain degree of irritation,—whether the primary cause of the disease, or one of the phenomena developed in its progress we shall not inquire —to which in due course of time, if the stage of collapse did not prove fatal, an inflammatory affection might and frequently did succeed.

Others have thought that the proximate cause or essence of the disease was, "a violont affection of the nerves, distributed on the inner surface of the stomach and intestines." Although Mr Jameson has succeeded in showing in the Bengal Report, that all the symptoms cannot be accounted for in this way, yet he has probably gone a step too far, when he denies that such an affection of the nervous system existed at all. It must, we apprehend, be conceded, that so violent a derangement of the surface of the alimentary canal as existed in the present instance, could not prevail without its nervous system being disordered. What that disorder may have been by no means appears. That it was the primary cause of the whole train of conspicuous symptoms is still less apparent. But there is no way in which many of the symptoms, particularly those affecting the general system, can be so satisfactorily explained as on the suppositon that some peculiar impression was produced either primarily or secondarily on the nervous expansion of the alimentary canal. To this topic we shall return when we treat of the general disorder that accompanied the intestinal derangement. At present we need only remind the reader, that the production of a peculiar impression on the nervous fibrils of the intestines has been assumed on strong presumptive evidence in the case of some poisons, between whose action and the phenomena of the spasdomic cholera there appears to be a very striking analogy.

The principle obstacle to be encountered in investigating the essence or proximate cause of this disease, is the difficulty of accounting upon any theory for the extraordinary discharge which took place in almost every instance from the stomach and bowels. Many opinions have been formed respecting the nature of the discharge. Some consider it the mere serum of the blood, separated by simple atony or relaxation of the intestinal capillaries; others consider it a slight modification of the ordinary intestinal mucus; others view it as composed of the last with a mixture of regurgitated chyle or unabsorbed chyme, and others again regard it as a new secretion.

Unfortunately, the information collected from so many quarters, however extensive and accurate it may be, proves upon this as upon many other points, too vague to direct the judgment. The remarks of the reporters are chiefly confined to its physical properties; and these merely enable us to say, that the first opinion cannot be correct; for although it was sometimes not unlike the mere serum of the blood, it was much more commonly of a totally different nature. We believe, we may likewise leave out of view the third opinion, that it is a fluid analogous to that of the Fluxus Cœliacus. For in the first place, it may be remarked, that a disorder of that description is of very problematical existence; and secondly, as Mr Jameson has properly hinted, it appears quite impossible to conceive, how all the chyme and chyle together could form such a prodigious quantity of the white creamy or flocculent matter as was frequently poured out.

The most obvious and certainly the most natural view, is that which considers the discharged fluid as merely a slight modification of the intestinal mucus. And if it is not such, there remains no other conclusion than that it is a new secretion. The question was to be decided between these two suppositions by chemical experiment. But it is most surprising, that among the many hundreds who have noted its sensible qualities with precision, not one seems to have had sufficient chemical knowledge or curiosity, to determine its actual nature by the effect of re-agents. The only experiment which has been tried upon it with that intention is one by Dr Scott of Madras, who found that sulphuric acid increased its whiteness, and partially coagulated it. A few other experiments made by Mr Annesley, with the view of illustrating the effects of remedies, may likewise be mentioned, although they do not throw much light upon its nature. He found that alcohol and diluted nitric acid coagulated the thick creamy sort of fluid; that ammonia, ether and camphor did not alter it; that tartaric acid rendered it quite fluid; that bile also dissolved it sensibly, and that calomel turned it to a dark greenish gray, and *appeared* to dissolve it, (p. 193). These experiments are not quite conclusive: At the same time, if correct, they will serve to render it probable, that the substance thrown out by the intestines was neither albuminous nor mucous in its nature, and consequently must have been a new secretion.

The inference to be founded on the determination of the foregoing point is a matter of some consequence in an inquiry into the proximate cause of the disease; for if the fluid was merely modified mucus, mere irritation of the alimentary canal will account for its discharge; but if it was a peculiar secretion, then we cannot but suppose, either that the minute vessels of the intestines had put on a new and peculiar action, under the influence of some novel and hidden cause, or that the blood, supplied for elaboration in these vessels, had been, as several imagine, very much altered in nature.

So much for the first set of symptoms of the disease, those namely connected immediately with derangemement of the alimentary canal. The next class of symptoms, which relate to disorder of the muscular system, need not occupy us long. They consisted of violent spasms, often affecting the whole muscles of the body, generally of a tetanic nature, and sometimes so purely such as to lead a careless observer to confound the disease with tetanus. This affection, one of the most prominent and alarming phenomena of the disease, is well known to be frequently owing to irritation in the alimentary canal; and consequently we need look no farther for its cause.

The disorder of the circulation is of a different nature. It was perhaps the most striking feature in the epidemic cholera, and has been regarded by several as constituting its very essence or proximate cause. This doctrine is espoused, though not to its full extent, by Mr Annesley.

The state of the circulation was always very remarkable. At a very early period, frequently from the first moment that the patient attracted attention, the pulse was excessively languid, and sooner or later it ceased to be perceptible in the extremities or in the temples. At the same time the blood was peculiary dark and viscid; that drawn from the temporal artery had the appearance of venous blood; the blood from the veins was almost black, and of an oily or even tarry consistence; and it could very rarely be drawn away but in a slow, languid stream. This state of the blood was attended, as might be reasonably expected, with extraordinary disorder of the respiration. Sometimes the breathing was much oppressed; and this symptom was satisfactorily accounted for after death by the excessive gorging of the pulmonary tissue with black gory blood. But this was not the most striking affection of the respiratory functions. Even when the breathing was full, free and rapid, and when consequently a sufficient quantity of air was inhaled, the oxygenizing process was carried on very imperfectly. For the blood was black and thick, the breath was peculiarly cold, the heat of the body was very much lowered, and the expired air, according to the experiments of Dr John Davy, did not contain the usual proportion of carbonic acid. These facts are all well ascertained; but there is a singular want of information upon the other properties of the blood. For example, as to its coagulability, one or two instances are mentioned in which it coagulated firmly and rapidly, and a few others in which it did not coagulate at all; and most of the Reports are entirely silent on the subject. No one seems to have had the curiosity to ascertain whether this apparently unoxidized blood could be rendered florid by exposure to the air. One person only takes notice that (in apparently the only case in which he attended to it) the crassamentum did not become florid on its surface as usual. This single fact, however, is of very little value. If it were correct, then it must be inferred, that the blood was not simply unoxidized but had undergone some essential change besides, which rendered it equally incapable of being oxidized and of supporting the functions. But many other circumstances conspire to prove that its dark and viscid condition was owing merely to the abstraction of its fluid parts by the intestinal discharge, and to a simple defect of oxigenation. Of these we need mention one only, the rapid change of the blood to its usual colour, accompanied with rising of the pulse, restoration of the temperature and abatement of the spasms, during the very act of bloodletting. Such a change is unintelligible on the supposition that the blood had undergone a positive change, but is easily understood, as will be seen when we notice the *rationale* of the treatment, if the blood was merely deficient in oxigenation.

Before proceeding to notice the pathological deductions which have been drawn by Mr Annesley and others from the condition of the blood, it may be well to remark, that this condition did not always prevail. In a very few cases it preserved its natural colour. But it was only in those in which reaction existed from the beginning, and continued till the blood was drawn, and in which the pulse was full, hard and frequent. These cases were so different from the generality and so few in number, that we are almost tempted to believe they were not cases of the true epidemic cholera. One person only of the host of observers who have reported their experience on the subject, did not remark any change in the colour of the blood as it flowed. (*Madras Reports,* 228.) But for reasons which it is not necessary to mention, little reliance can be placed on this observation. We may therefore take it for granted that the blood was with a few exceptions imperfectly oxidized, and when such, became florid if its quantity was diminished.

To this state of the blood several persons, and among these Mr Annesley, have endeavoured to refer the leading symptoms; and some have even sought in it for the primary cause or essence of the disease.

There is no doubt that a deficiency in the oxigenation of the blood will account admirably for many of the leading symptoms. Thus, as Mr Annesley remarks, it will explain the appearance of venous congestion met with in almost every part of the body, the cessation of the pulse, the coldness of the skin and of the breath, and the stupor which prevailed towards the close. The Madras Report likewise suggests that it will account for the spasms, as the presence of dark blood in the arteries is known to produce them on other occasions. Perhaps it will farther account for the discharge of cold, clammy sweat from the skin; at least this is an almost constant phenomenon towards the close of other diseases in which the blood gradually acquires a venous character. And lastly, it explains with precision the manner of death, which every one who appears to have viewed it with a physiological eye compares to gradually increasing asphyxia.

The question will then occur, how this extraordinary condition of the blood is produced, and how it stands related to the other leading symptoms of the disease, to wit, those arising from the affection of the alimentary canal.

The opinion of most of those who have touched on the subject appears to be, that the condition of the blood and circulation is caused by the disorder of the alimentary canal. But others, and among these we apprehend we must rank, though not very decidedly, Mr Annesley, imagine that the condition of the blood is the primary cause or essence of the disease, and produces not only the symptoms ascribed to it above, but likewise the derangement of the stomach and intestines, and the stoppage of the urinary, biliary, and salivary secretions; and an attempt has even been made by Mr Annesley to trace the origin of the altered state of the blood to a peculiar *epidemic constitution* of the body existing more or less in all.

"For the last four or five years," says he, "my attention has been particularly attracted to the peculiarly thick and black appearance of the blood in almost every case where I had occasion to perform the operation of venesection, whether in cholera, dysentery, fever, hepatitis or rheumatism. I mention rheumatism, because it was a case of this disease in which I was, in 1817, first struck with the black condition of the blood.* - - - -" "This appearance, however, - - - - was passed unnoticed till my attention was arrested by its continual presence, - - - - and by its nearly uniform occurrence among all classes of persons of whatever description, from the most delicate female to the most robust and hardy soldier." - - - - p. 140.

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* This fact is the more pointed, that in general the venous blood in [acute] rheumatism is peculiarly florid.

"That this very unnatural and uncommon condition of the blood, which appears to me to be caused by an unusual accumulation of carbonaceous matter, must lay the foundation for formidable congestive disease, cannot, I think, be disputed." p. 142.

We confess we have some difficulty in understanding how such an epidemic constitution of the blood can be produced. The arterialization of the blood is a chemical process which must be carried on so long as the blood is capable of oxigenation, is freely transmitted through the lungs, and is there exposed to a sufficient quantity of properly oxigenated air. None of these conditions is alleged to have been wanting; and accordingly Mr Annesley and several others have assumed, that the nervous system is also concerned in the arterialization of the blood, and that some peculiar impression on the nervous system occasions a defect in that process. We are not aware, however, of any facts which prove the arterialization of the blood to be primarily dependent on the nervous system at all; yet, on the other hand, we know not that any of the late admirable investigations into the physiology of respiration prove that it is not; and at all events, however unaccountable it may be, Mr Annesley's fact, that a defective oxigenation of the blood did prevail *epidemically*, stands undenied.

The next step in this investigation would naturally lead to the inference, that an accidental increase of this constitution of the blood, produced by the application of various *remote* or exciting causes, was the essence of the disease, and the cause of all its symptoms. This notion has been very confidently taken up by several of the Madras Reporters, who were not aware of the pre-existing constitution of the blood remarked by Mr Annesley. (*Madras Reports*, 222, 286.) But he himself is evidently too cautious an inquirer to be overwhelmed and hurried along by his observations, however singular and important they are. For, in a different part of his work, he remarks—"That this state of the blood was the first material derangement consequent on the invasion of the efficient cause of the malady, I shall not contend; but that it was one of the earliest links in the chain of effects consequent to that cause, and that it afterwards tended, by a necessary and evident process, to heighten and perpetuate the derangement whence itself sprung, I have not the least doubt." p. 127.

It is very difficult, indeed, to conceive, how a deficient oxigenation of the blood could produce all the symptoms of the Epidemic Cholera. We have already shown how well it accounts for many of them. But will it explain the stoppage of the secretion of urine, bile and saliva, and the profuse discharge of matter thrown out from the intestines? We are not acquainted with any fact which will uphold such a doctrine. But farther, it appears undoubted that the intestinal discharge frequently, perhaps always, existed previous to the black tarry condition of the blood; and sometimes, though certainly but very rarely, the former existed long and finally ceased, without the latter appearing at all.

On these accounts several have conceived, that the state of the blood, instead of causing the disorder of the intestines, was produced by it, and that the intestinal disorder acted on the circulation through the nerves. We can easily understand that this may happen; at least there is no other way of explaining a similar state of the circulation and of the blood in some other abdominal diseases that are more familiar to the European physician. In Enteritis, in the affection of the alimentary canal induced by some poisons, and in the simplest of all irritating causes, rupture or other mechanical injury of that canal, a state of the circulation is induced which approaches very closely to that observed in the Eastern Cholera.

On the whole, therefore, the most rational theory we can form regarding the nature of this most extraordinary malady is, that its exciting cause, whatever that may have been, operated in the first instance on the alimentary canal, that it had a tendency to bring on inflammation, but that this was counteracted either directly by the nervous injury with which it was accompanied, or indirectly by the effect of this nervous injury on the circulating system already prepared for the impression by its state of imperfect oxigenation; that it was only after the removal of the collapse thus induced that the phenomena of proper inflammation developed themselves; and that even then the inflammation was modified either by the process the system had undergone, or by the original cause still existing to a certain degree, and tending to operate afresh. This theory, if it assumes too much by supposing the existence of a peculiar nervous impression, has at least the advantage of being consistent throughout, and of explaining every well ascertained fact regarding the disease.

It will presently be found that the explanation now given of the symptoms accounts with great precision for the effects of the principal and most efficacious remedies.

The treatment of the Epidemic Cholera forms one of the greatest and most undoubted triumphs of modern physic. Unfortunately too many opportunities occurred of ascertaining the extent of its ravages when uncontrolled by art; while the most satisfactory evidence has been likewise furnished of the impression made on it by medical treatment. Many extracts in proof of what is now said might be taken from the Government Reports; but the following will be amply sufficient. In the Zillah of Nellore,—where the disorder seems to have been peculiarly prevalent, and where the government, with most praiseworthy humanity, used all possible means to relieve the inhabitants, by sending European surgeons and native vaccinators to instruct the village doctors,—it appears that several districts received ample assistance, while others either did not obtain it, or, from motives of superstition or other prejudices, would not use the aid offered. According to the Reports derived from the police and the doctors and vaccinators, of 1507 who did not receive aid, 1255, or five-sixths perished, while of 5453 who received most of them ample assistance, only 981, or betwixt a fifth and a sixth died. (*Madras Report*, p. 46.) In the army the proportion of deaths was somewhat greater, probably on account of the cases occurring chiefly in marching corps, when the system was ill prepared to resist the attack of so formidable a disease, and when it was impossible to command the necessary conveniencies. In the Madras establishment, of 2287 Europeans who were attacked, in four years, 425, or 18 per cent. died; and of 10,885 natives, 2986, or 27 per cent. It may be added, that several surgeons express their conviction, that in its severe form, attended with early collapse, it is almost if not altogether inevitably fatal, without the speedy aid of art.

According to the statements contained in many of the reports, and the opinion delivered thereon by Mr Scott, it appears highly probable that very trivial remedies may have an important effect on the disorder when it is just commencing. There is no other way at least of accounting for the beneficial effects ascribed by many to various spirituous liquors, to ether, ammonia, laudanum, and several other stimulants, which in the more advanced or formed stage, were certainly found by most to be of very little advantage.

When the disease was fully formed, there were evidently two objects to be held in view for regulating the method of cure; the one, to check the derangement of the alimentary canal, the other to counteract the state of collapse which pervaded the whole system. To effect the latter, powerful stimulants, such as brandy, arrack, ether and ammonia, were naturally resorted to in the first instance; and to attain the latter, the East India physician as naturally betook himself to the mysterious and panaceal virtues of calomel, aided by the more intelligible operation of laudanum. Such was the essence of the method of cure conceived by Mr Corbyn of the Bengal Establishment, and adopted for some time by the Medical Boards and acting surgeons of all the Presidencies. For the particulars of his method we refer the reader to our Review of the Bombay Reports in a former Number, (Vol. xvi. 467.) It was afterwards found, however, in various quarters, that this mode of treatment was not so successful as its propounder had conceived; and partly on account of the failure of all other means, partly from reasoning on the nature of the disease, several surgeons were led independently and simultaneously to resort to the lancet.

We need not occupy the reader's time by discussing the various objects held in view by different persons, in having recourse to a remedy at first sight so incompatible with the disease. Some used it because they thought the disorder inflammatory; others, and among these Mr Annesley, used it for the vague general purpose of relieving internal congestion; and several do not seem to have known well what was their object. But all appear to us to have mistaken its true operation. The main cause of danger was the gradually increasing deoxidation of the blood, and the consequent poisoning of all the organs in the body. By diminishing the volume of the blood, and the quantity of its globules, the heart, as some have hinted, was probably enabled to contract more vigorously on the remainder; but also, which was of greater consequence, the power of arterialization remaining in the pulmonary system became sufficient to oxidate what remained, and consequently to restore red blood to the poisoned organs. That this change in the nature of the blood did ensue is agreed upon all hands. When the vein was opened the blood trickled slowly down the arm in a dark tarry stream. If it ceased to flow when a few ounces were drawn, and when it still retained its deoxigenized appearance, the patient was irretrievably gone. But more generally it assumed by degrees a more and more florid tint, and flowed more and more freely, while at the same time the pulse rose, the pain of the belly abated, the spasms became less violent, and the skin began to recover its temperature. With one or two exceptions every reporter agrees as to these facts. They are likewise almost unanimous in commending the remedy. Those who object to it advance no positive objection, but merely the negative one, that blood sometimes could not be procured, and that when they succeeded in drawing away small quantities it did no good. On the other hand, a vast body of unconnected practitioners agree, that when a large quantity could be drawn success was almost certain; and in a very few rare instances only was it followed by sudden death, which many, viewing the outward symptoms only, might very naturally be led to dread. No rule is laid down by Mr Annesley as to the quantity which should be taken away, except that it must be allowed to flow till the pulse rises and the blood becomes red. These objects appear to have been seldom attained till 20 ounces at least were withdrawn, and much more frequently the quantity amounted to 30 ounces and upwards. In one instance Mr Annesley withdrew in the course of five hours 46 ounces at two venesections, and 57 ounces by leeches. The case was a very severe one, and recovered; a clear proof that there is nothing in the essence of the disease to render the depleting practice dangerous. On the other hand, the abstraction of a small quantity seems to be of little use; and on that account the remedy fell into some disrepute with several practitioners. One gentleman declares it of no use, but he rarely succeeded in drawing 10 ounces, and never more, (*Madras Reports*, 84); and another lost all his patients in whom he used it, even although the blood changed its colour; but like the former, he was contented with withdrawing 10 ounces, (Do. 207). Whenever blood was withdrawn freely its success was beyond that of every other remedy. Thus, Mr Boyd, assistant-surgeon at Mhow, in the worst possible cases bled his patients to deliquium, and lost only 2 out of 28, (Do. 95); and in more favourable circumstances, Mr Annesley was even more fortunate; for of 50 patients removed into his hospital from the General Harris East Indiaman, he did not lose one, (p. 172). Many other incidents equally satisfactory might be quoted. Sometimes the return of the signs of collapse render a repetition of the bloodletting necessary.

When blood cannot be procured with the lancet, then it is to be tried by means of leeches, which in India are very remarkable for their size and power, and constitute a remedy, of whose efficacy we can have no conception, from its use in European practice. * Their effects were precisely the same with those of venesection. When both means fail to abstract blood, then there is no resource but to treat urgent symptoms; but we must not lose sight, nevertheless, of the possibility of withdrawing blood, even when we have failed at the beginning of the disease. For, "an opportunity sometimes offers, in the advanced stage, indicated by a struggle or effort of the system to overcome some resisting power, and is a most auspicious symptom which should never be overlooked. This reaction indicates that the constitution is making an effort to restore the circulation, but is unable to do so till assisted by the abstraction of blood, which abtraction aids in removing that oppression which it has not power of itself to overcome." (p. 181.) In this conjuncture, however, it is necessary to proceed with caution, as several appear to have died suddenly, in consequence of too bold a use of the lancet. They were all, however, in very desperate circumstances at the time.

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* Mr Annesley has found by frequent and careful measurement, that the leeches he used withdrew on an average an ounce at least. Those used in this city do not draw the 4th part of that on an average.

The remedies subsidiary to venesection which have been recommended by Mr Annesley, and almost every one who has expressed an opinion on the matter, are frictions, rubefacients, external heat, and internal stimulants. The other outward applications are directed rather against the intestinal disorder than that of the circulation. Of the three remedies we have mentioned, the only one which requires notice from us is external heat. The first method of using it was in the form of the water-bath. But independently of the insuperable prejudices which the natives of Hindostan have against it, a more valid objection exists in the difficulty of applying it without subjecting the patient to dangerous fatigue, and in the positive harm resulting from the powerful impression made by the hot water on the cold skin. A much better way of applying heat is by warm flannels, the sand-bath, or chauffers under the patient's cot. The internal stimulant chiefly used and recommended by Mr Annesley is a mixture of camphor, ether and ammonia.

The second object held in view in establishing the method of cure was to check the derangement of the intestinal canal. The chief remedies employed for this end were calomel and opium, resinous purgatives, and blisters and corrosives applied to the skin of the abdomen. We shall close the present article with a short account of the effects of these remedies, and particularly of calomel.

The first person who used calomel in the present disease, in the methodical manner now practised in India, was Mr Corbyn of the Bengal Establishment. His object, as will be seen from our account of his method in a former Number, * was to produce a "sedative effect, allay vomiting, remove spasm, and send the patient to sleep." Mr Annesley concurs in these views of the action of calomel. Indeed, almost every East India surgeon appears to agree, that although calomel in small doses is an irritant, yet in the quantity of a scruple it is a direct sedative. Without entering into the merits of this question at present, it may nevertheless be simply stated, that better proof of such an action is required than what has been advanced by any of the surgeons of the East; for all of them combined the calomel

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* Vol. 16. p. 467.

with large doses of laudanum or solid opium, even Mr Annesley, while he undervalues that drug, always gave it; and truly it is difficult to comprehend how the sedative effects of calomel are to be demonstrated under such circumstances. A very different, much simpler, and much more novel view of its action, however, has been hinted at, if not absolutely adopted by Mr Annesley, in a different part of his work. It will be remembered, that from some chemical experiments he made on the viscid matter thrown out upon the intestines, he inferred, that bile has the power of dissolving it, and that this property is even to a certain degree possessed by calomel. Its operation is, therefore, referred to a simple chemical process. It softens the clammy matter attached to the intestines and closing the biliary duct. Thus, on the one hand, a passage is cleared for the bile, and on the other, that fluid is aided in softening the offensive matter, in clearing it away, and consequently in presenting the surface of the intestine unincumbered to the operation of other remedies. These singular views do not want the confirmation of several very striking facts. Thus, calomel gives the white matter out of the body greater fluidity, and turns it to a dark grayish-green; and calomel and bile together render it green, and still more fluid. Now the very same appearance, accompanied with the separation from the intestine of the matter thus changed, and of the changed matter only, was found in the bodies of those who died after calomel had been given; and the first favourable symptom (relative to the state of the intestines), and a symptom which was followed by almost certain recovery, was the discharge by the rectum of a matter of the same appearance and character. We are most willing to leave the author in full possession of the merit of his ingenious and novel view. But we cannot help informing him, that this alleged action of calomel is wholly at variance with every thing known regarding its chemical relations to the common animal fluids and solids, and must therefore assume a very peculiar chemical character for the cheesy or slimy matter thrown out by the intestines in this disease. Here, again, we cannot but lament the want of a little chemical knowledge, or chemical curiosity. It would have been no very difficult task to have determined whether there was actually any chemical change effected.

In a subsequent part of his work, Mr Annesley examines this curious subject still farther, and he finds by experiments on dogs, that a dark gray colour is produced by mixing calomel with the mucous matter which lines their intestines: he adds, that the colour resembles that produced by adding ammonia to calomel; and therefore that it appears probable the secretion of the intestines operates in the same manner as that alkali. But why, may we ask, was not the 'probability' of this assumption put to the test of experiment? In the account he gives of his experiments on dogs, several other facts are mentioned, which he conceives throw light on the action of calomel upon the body. When a few large doses of calomel were given, he found the flow of the bile to be most obviously increased. This is a valuable fact, of more weight than thousands of cases of an *apparent* increase caused in the human body. We are not sure we can concede to him so readily the merit of accuracy in his researches into the effects of calomel on intestinal vascularity. By experiments on dogs with various doses, he thinks he has found, that, in scruple doses, it diminishes vascularity in the mucous coat of the intestines, and that the diminution increases with the number of the doses. There is a source of fallacy here which must be accounted for before his conclusions can be admitted. He assumes as the result of personal observation, that the natural state of the alimentary mucous membrane is one of very high vascularity: and he has given plates of its natural state, in which the intestine is represented as dark as a black currant, and the stomach like a raspberry. Assuredly we have seen these parts in healthy dogs many a time, and have had occasion to examine them carefully too; yet we have never seen any thing in the least analogous. Still his experiments, which are illustrated by several beautiful drawings, seem to admit the conclusion, that when congestive vascularity exists, it diminishes under the use of calomel, that it then assumes more of an arterial tint, and at length gradually vanishes. And in this way, perhaps, may be explained its evident good effects in the advanced stage of epidemic cholera, when the state of collapse is removed, the intestinal secretion evacuated, and the membrane tending, as it certainly often does, to put on an inflammatory action.

The way in which Mr Annesley gave this remedy in the cholera, was in doses of 20 grains, repeated three or four times every two hours, accompanied each time with two grains of opium, and followed by resinous purgatives.

Blisters, and even corrosives, were used, both to counteract the intestinal derangement, and as general stimulants. The blisters used were cantharides and hot water, the corrosive diluted nitric acid.

After the state of collapse was removed, and the intestines, to appearance at least, discharged of their noxious contents, subsequent symptoms often occurred, which required to be carefully watched, and actively treated. These were referrible chiefly to two affections, a low inflammation of the intestines, and congestion, if not actual inflammation, of the brain. Both were often exceedingly obscure, and required more inquiry for their detection than mere questioning of the patients. They were almost always successfully treated by leeches, calomel, and purgatives. They arose, as it appeared, from the intestines not being thoroughly cleansed.

One other circumstance only, with regard to his method of treatment, our space will permit us to allude to. Almost every practitioner without exception condemns cold drinks, as fraught with extreme danger [Madras Reports, *passim*], denied them most perseveringly to his patients, substituted warm stimulant drinks, and even of these did not allow a very liberal portion. "Anxious at all times," says Mr Annesley," to consult the dictates of nature in the treatment of disease, where there is no decided and manifest objection; and as I saw no good reason why warm drinks should be exclusively essential in this disease, more especially as there was so strong a predilection for cold drinks on the part of the patient, I resolved to step out of the beaten path, and gave my patients the nitric acid agreeably diluted; and I was gratified to find not only that no bad consequences followed, but that it was a most pleasant beverage to the patient, relieving that most distressing and urgent symptom already insisted on, the burning sensation. This, therefore, became the general drink in the Hospital in this disease." p. 198.

On a future occasion we shall recur to some of the other important topics embraced by Mr Annesley's work, and the Reports of the three Presidencies.

IV.

The Works of Matthew Baillie, M. D.; to which is prefixed, an Account of his Life, collected from authentic sources. By JAMES WARDROP, Surgeon Extraordinary to the King, &c. &c. &c. In two volumes. London, printed for Longman Hurst, Rees, Orme, Brown and Green, Paternoster-Row. 1825.

Lectures and Observations on Medicine. By the Late MATTHEW BAILLIE, M. D. London, printed by Richard Taylor, 1825. Large octavo. pp. 242.

THESE volumes form the only literary remains of the late eminent physician of the metropolis. Of the two first volumes published by Mr Wardrop, the greatest part of the contents have been already before the public in one form or other. The miscellaneous papers have been already published at various times in the Transactions of the Royal Society of London, in the Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, and in the Medical Transactions of the College of Physicians, and therefore cannot be ranked among the legitimate objects of consideration in the present article. On the appearance of these papers, however, in the volumes before us, we have only two observations to offer. The first is, that no mention is made of the works in which they originally appeared, or the time of publication; the second, that they are arranged upon no fixed principle that we can discover, but are printed apparently at random entirely. As many professional readers may not be in the habit of perusing either the Transactions of the Society for the Improvement of Medical knowledge, or those of the College of Physicians, much less those of the Royal Society, which are daily becoming less medical, they may naturally suppose that these Essays are now before the world for the first time. It would have been useful, therefore, if the present editor had appended to each Essay the title of the work in which it made its first appearance, and the date of communication or publication. This simple plan would further have suggested a mode of arrangement as good as any other,—that of chronological succession, which is sometimes not merely a matter of curiosity, but of considerable utility to the student who wishes to be familiar with the literary history of his profession, and to have a distinct view of the labours of the author whose works he reads. With the view of supplying this omission, we subjoin the following tabular list of the papers of Dr Baillie, according to the order of publication and the works in which they appeared.

	Page of the original work.	Date of Communication.	Page of Mr Wardrop's Edition.
PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY OF LONDON.			
Vol. LXXVIII. London, 1788. Part II			
An Account of a remarkable Transposition of the Viscera. In a Letter to John Hunter, p. 350,	350	1788	148
Vol. LXXIX. London, 1789.			
An Account of a particular Change of Structure in the Human Ovarium, communicated by John Hunter, p. 71, - - - -	71	1789	137

TRANSACTIONS OF A SOCIETY FOR THE IMPROVEMENT OF MEDICAL AND CHIRURGICAL KNOWLEDGE.

	Page of the original Work.	Date of Communication.	Page of Mr Wardrop's Edition.
Vol. I. London, 1793.			
On the Want of a Pericardium in the Human Body	91	1788	44
Of Uncommon Appearance of Disease in Bloodvessels Obliteration of Veins, and Ossification.	119	1789	28
Of a remarkable Deviation from the Natural Structure of the Urinary Bladder, and Organs of Generation of a Male - - -	189	1790	166
Case of Emphysema not proceeding from Local injury	202	1791	66
Vol. II. London, 1800.			
Account of a Case of Diabetes, with an Examination of the Appearances after Death -	70	1795	94
Account of a singular Disease of the Great Intestines, (a portion of colon discharged by stool) -	144	1796	121
Account of the Case of a man who had no Evacuation from the Bowels for nearly fifteen weeks before his Death - - - - - -	174	1797	112
Vol. III. London, 1812.			
On Embalming Dead Bodies - - -	7	1804	175
Account of several Persons who had the Measles twice	258	1808	70
Additional Instances of the same - -	263	1810	84
Three Cases of Inflammation of the inner (mucous) membrane of the Larynx and Trachea -	275	1809	54

MEDICAL TRANSACTIONS PUBLISHED BY THE COLLEGE OF PHYSICIANS.

	Page of the original Work.	Date of Communication.	Page of Mr Wardrop's Edition.
Vol. IV. London, 1813.			
Case of Hydrocephalus, with separation of the bones of the Cranium - - -	1	1806	12
Case of Internal Hydrocephalus, with uncommon symptoms - - -	9	1806	17
Observations on a strong pulsation of the Aorta in the Epigastric region - - -	271	1812	22
Vol. V. London, 1815.			
Upon a Case of Stricture of the Rectum, produced by spasmodic contraction of the Internal and External Sphincter - - -	136	1814	127
Some Observations respecting the green Jaundice	143	1814	89
Some Observations on a particular Species of Purging (the calcareous supposed) - -	166	1814	132
Vol. VI. London, 1820.			
Some Observations upon Paraplegia in Adults -	16	1817	1

The morbid anatomy which constitutes the second volume has been long well known, and may now be regarded as an indispensable book of reference to the student and practitioner of medicine. We must not forget however to remark that, it is preceded by preliminary observations on diseased structures, in which the judicious reader will easily trace the same talent for pathological inquiry and description, which distinguished the treatise on the morbid anatomy of the human eye, and the best work on Fungus Hæmatodes yet extant.

The principal novelty, therefore, in the publication of Mr Wardrop, is the interesting biography of Dr Baillie at the beginning, and the dissections at the end of the first volume. This is not the place for entering into the details of the life of Dr Baillie; and it is enough to say, that it is written with much elegance and taste, as well as with an intimate knowledge of the habits and disposition of his subject by Mr Wardrop. The character of Dr Baillie is clearly and forcibly delineated; and though the history is not eventful, it will be perused with interest by the professional reader.

The dissections, though short, are pretty numerous; but as they appear to have been merely notes of the appearances of individual cases, without any particular arrangement or express purpose, they do not form a very fit subject for especial consideration here. We have only to remark, that the student will here find several interesting examples of pathological facts;—for instance, the state of the intestines from fatal purging, (p. 210), adding one to the many proofs of that symptom being the effect of inflammation of the intestinal mucous membrane; obliteration of the right carotid artery, recorded many years ago,—to show that an animal may live without circulation through one carotid artery, and to prove the possibility of the spontaneous cure of aneurism; an instance of extra-uterine conception; two dissections of an impregnated womb; an example of the fatty degeneration of muscles, and some others of more ordinary occurrence. As these cases of dissection are further rendered accessible to our readers in the volumes of Mr Wardrop; our duty is reduced to the simple announcement of their character and the recommendation of their perusal.

It is very different with the volume of Lectures and Observations, which were directed by the revered author himself to be printed, but not published. The name of the author of the Morbid Anatomy has stamped so high a value on every thing which he has written, that we can scarcely doubt that public opinion will coincide with our own in attaching the highest interest to that which has appeared last, and the contents of which are least accessible to medical readers. To this therefore our attention on the present occasion will be principally confined.

Of the several kinds of medical publications which are daily issuing from the press, none are calculated to claim greater attention, awaken higher interest, or be received with more sincere confidence, than those which, at the death of a great and distinguished physician, are bequeathed to the world as the last remains of long experience, profound skill, and unabated regard for the welfare of his fellow-creatures. When a physician publishes during life, he is generally supposed to have some temporary interest in view; he is always exposed, however unjustly, to the suspicion of writing for the purpose, as it is termed, *of making himself and his qualifications known;* and of showing that he is not only capable of instructing his brethren and promoting the advance of professional knowledge, but is in possession of stores of information which may be useful to those who apply for his opinion and advice. Even when a physician becomes an author in the decline of a long and active life, he is still supposed to derive considerable gratification from the conscious pride of showing that he can combine the literary with the professional character, from the well-earned fame which his writings may acquire, and from the confidence with which his opinions are quoted, and his authority adduced.

Whatever grounds there may be for these opinions in the instance of the writings of a living physician, it is certain that they can rest on no solid foundation in the case of posthumous works. Temporary views have then ceased to operate, and temporary purposes can no longer be served. When the author has ceased to be among his fellow-men, and continues no longer to be the object either of competition, or of admiration;—when he has ceased even to be the last hope, on whom the dying patient can lift his closing eyes, it must avail him little indeed to communicate to the world those gleanings of knowledge, which the experience of years has enabled him to acquire in opposing the ravages of disease and mitigating the agony of mortal sickness.

The posthumous writings of the physician may demonstrate his skill in detecting the nature of disease, and his promptitude in using the proper means of removal; but they can no longer command or increase the public confidence in his favour, or put that skill in greater or more general requisition. The posthumous writings of the physician may show how much natural sagacity, improved by long study and habitual observation, he brought to the management of disease; but they tell at the same time the melancholy truth, that neither his sagacity nor his experience are accessible to his fellow-creatures, or available in diminishing the number and severity of their sufferings. The posthumous writings of the physician may add to the actual stock of useful information, and thus merit and receive the sincere applause of grateful survivors; but their praises neither reach his ear nor touch his heart; and in whatever degree the results of his experience may contribute to diminish the sum of human misery, he can no longer derive gratification from the gratitude either of the healer or the healed;—either of him who administers the remedy, or of him to whom it is administered. It may even happen, that the posthumous writings of an eminent and celebrated physician may tarnish the lustre of that reputation which they were intended to brighten; and as no one can be sure that he is to be successful in avoiding that error into which the greatest geniuses have fallen, the vain hope which the author fondly cherished of living in the memories of men when his mortal presence had ceased to exist, may be irrecoverably blighted; and the hard-won reputation of a life of incessant exertion and unwearied diligence, may be sacrificed by a single act of indiscretion.

From circumstances of this description, the posthumous productions of a physician of any eminence acquire a singularity of character, which belongs to few other literary performances; and it is only when the certainty of the small and insignificant advantages is contrasted with the contingent greatness of the risk, that we can form a just estimate of the benevolence of those motives which actuate the author in directing their publication. Considered in this light, posthumous writings must be regarded as the freest gift that can be bequeathed to the world,—a boon in the strictest sense of the word, to posterity, without the certainty even of receiving approbation, and that approbation confined to the small number only who are competent to appreciate the value of the bequest. It is undoubtedly to the influence of reasons of this nature, that we are to ascribe the rarity with which the writings of medical authors appear after death, and especially the infrequency with which we see them directed to be given to the world by the express wish of the author.

If there is any book which could more readily than another suggest the foregoing observations, it is undoubted the present. To say that Dr Baillie was a physician of skill, of great experience, and of high celebrity, is to omit the most important and peculiar features in his character. That he had all these qualifications, is too well known to admit of any doubt; but he shared them in common with many other physicians, who have justly received the confidence of their contemporaries and the applause of posterity. The peculiarity of the character of Dr Baillie, consisted in his acquiring this distinction by no other means but unwearied application in the attainment of medical knowledge, and the diligence and judgment with which he cultivated the science of morbid anatomy. Without that engaging manner or happy address to which many are indebted for worldly success, reluctant or disdaining to cultivate any of those arts which are known both to gain and to preserve patrons, unconnected with any party, either in church or in state, and relying solely on that patronage which science was to procure him, Dr Baillie came, not indeed at an early period of his professional career, nor yet late, into the greatest degree of public confidence and professional fame ever possessed by any individual. He was not only a scientific physician in the strictest sense of the word, but he was a practitioner of great practical experience. His success was a complete contradiction to the vulgar error, that a very scientific anatomist makes a bad practitioner; and though his intimate acquaintance with the structure and morbid deviations of the human body, taught him more certainly than others, the powers of remedies, and the limits within which their operation is restricted; yet his good sense and experience, while it kept him from the indifference of empiricism, was also a protection against the diffidence which too often unconsciously misleads the philosophical anatomist. From an observer of this character, every hint must be valuable; and the third part particularly of the volume before us, furnishes ample proof of the advantage which the healing art might derive from physicians such as Dr Baillie, recording the results of their experience.

The present volume consists of three divisions; first the Introductory Lecture to the Course of Anatomy, delivered in Great Windmill street, in two parts; secondly three Gulstonian Lectures read before the College of Physicians in May 1794, on the Anatomy and Physiology of the Nervous System; and thirdly, some brief observations drawn from the experience of the author, upon a considerable number of diseases.

Of the first of these parts, it cannot be expected that the matter should be very new or original; and it is accordingly chiefly distinguished for the extreme simplicity of its manner, and the unaffected beauty of the style. The arrangement is clear, the language precise and perspicuous, the information well selected and apposite, and the whole lecture shows the most complete and masterly acquaintance with the subject.

A course of anatomical lectures may be commenced in various modes according to the views of the teacher, or the supposed capacities of the pupils. Dr Baillie has chosen the history of his science; and without attempting more minute detail than is compatible with the usual duration of a single discourse or two, he has given a very neat philosophical sketch of the rise and progress of Anatomy, from the traditionary anatomical information of the Egyptian priests and embalmers, to the days of Harvey and the two Hunters.

The history of Anatomy may be divided in various modes; but a division not unnatural is formed by the great era of the discovery of the circulation, and by the analogous discovery of the uses of the absorbent vessels, first started by William Hunter, and afterwards completed by John Hunter, Mr Hewson, and Mr Cruickshank. The history of the progress of the science to the discovery of the motion of the blood by Harvey, is shortly but perspicuously traced in the first part of the lecture; and in the second part there is an equally clear account of the progress of anatomical research in unfolding the disposition and structure of the lymphatics, and of the successive steps by which the uses assigned to that system of vessels in the Hunterian school were converted into a physiological doctrine. It is not our intention to follow the steps of Dr Baillie in a minute abstract of this lecture; for it ought to be perused by every one who wishes to form a just idea of its merit. We shall merely attempt to communicate some idea of the general spirit which reigns through the discourse.

After a short sketch of the anatomists of the Greek school, from Hippocrates and Aristotle to Herophilus and Galen, and a few observations on the Arabians and Avicenna, Dr Baillie enters at once on the history of Vesalius at the revival of literature in the beginning of the sixteenth century. He then proceeds to explain shortly what is meant by the term *circulation* in physiology, and forthwith to trace the steps by which anatomists discovered the uses of the arteries and veins, and by what means they inferred the motion and course of the blood through these canals. In his account of the opinions of MICHAEL SERVETUS he quotes from his work on the Divine Trinity the celebrated passage in which that unfortunate scholar, the victim of religious bigotry and party zeal, describes so clearly the course of the blood, from the right auricle of the heart through the lungs to the left auricle; or what has since been denominated the *less circulation*. He then adverts to the discovery of the same circulation through the lungs made by COLUMBUS; and of the valves of the veins first taught by FABRICIUS AB AQUAPENDENTE. This leads immediately to the account of his illustrious pupil, the English physiologist, whose name has acquired everlasting renown by establishing the most beautiful and extensive generalization of facts with which the knowledge of the animal economy has ever been enriched. The conclusion of this part of the discourse forms such a happy example of the unaffected neatness which constitutes its predominant feature, that our readers will peruse it with pleasure.

"It is pleasant to observe how naturally he speaks of his first apprehension of the circulation. 'I began,' says he, 'to think whether there could be any motion of the blood as in a circle; which I afterwards found to be true, and that the blood is propelled from the left ventricle of the heart, through the arteries, to every part of the body, as it is to the lungs from the right ventricle, through the arterious vein, (now the pulmonary artery), and that it again returns through the *vena cava* into the right auricle, as it returns from the lungs into the left ventricle, through the venous artery, (now the pulmonary veins), as has been already mentioned.'

"This passage of Harvey, which is very faithfully translated, contains a most accurate and pointed description of the circulation, without any ambiguity, and would of itself be enough to establish his claim to this discovery; but he has extended it into a considerable work, by a great variety of experiments and arguments, so as to leave nothing to be done by his successors."

"In this manner was completed the discovery of the circulation, perhaps the greatest in the animal economy; and if it may appear to any person viewing it at present, that, after what had been done by Servetus, Columbus, Caesalpinus, and Fabricius, it required no very extraordinary effort of genius, it ought to be recollected how disposed the mind is to be governed by former opinions, and how little apt it is to be observant of what is unknown."

"When the discovery of the circulation became generally diffused, it met with an unfavourable reception from many anatomists. Some of them denied the truth of what had been asserted; and others ascribed the discovery to some of the ancients. Arguments and experiments soon overcame the first kind of opposition; and the other was easily seen through, having arisen from those feelings of envy, which will not bear even a merited superiority. But all is now hushed, Harvey is in full possession of the discovery, and the envious clamour of contemporary anatomists has ceased for ever."

The second part of the discourse traverses a more extensive range. It consists of a survey of the discovery of the lymphatics, and of the function which they perform; a short account of the advantages which the science of anatomy has derived from the use of the microscope, and the arts of preserving and injecting animal bodies; some interesting observations on the life and scientific labours of William Hunter; and concludes, very properly, with remarks on the benefit which an intimate knowledge of the structure of the human body confers on the art of distinguishing and healing diseases.

Every physiological truth consists of two things,—a collection of anatomical facts ascertained by dissection, exposition, preparation and injection, and, if requisite, by microscopic observation;

and a train of inferences or conclusions drawn by means of observation or well-contrived experiments. This is precisely the case with the Hunterian doctrine of absorption. Dr Baillie, after premising a short account of what is meant by the lymphatic vessels, notices the several facts which the research of anatomists had collected, by Erasistratus, Asellius, Pecquet, Veslingius, Peiresc, Van Horne, and Olaus Rudbeck, in the years 1651 and 1652. To this succeeds the account of the doctrine taught by Hunter in 1747, that the lymphatics were not continuations of arteries, as was believed by preceding anatomists, but actual absorbents, arising from every surface and interstice of the human body, and forming, with the lacteal vessels of the intestinal canal, a distinct system of vessels, possessing and exercising the vital power of absorption.

> " This he proved by the strongest arguments, drawn as well from the minute injection of blood-vessels, while the absorbents were not filled, as from the resemblance of the lymphatic to lacteal vessels, and their termination in the same common trunk. This discovery has been claimed by a celebrated professor of anatomy in Edinburgh; but I shall avoid entering into the dispute. It is enough to say, that Dr Hunter taught this doctrine in the year 1747, six years before that professor declares himself to have made the discovery. Dr Hunter has, therefore, an undoubted claim to priority, whatever praise may belong to any other person for having made the same discovery without assistance." p. 53 and 54.

Dr Baillie evidently regarded the Hunterian doctrine of absorption as a thing firmly and incontrovertibly established. He represents it as admitted unanimously by physiologists, and entertains no doubt of the competency of the few experiments performed by John Hunter to obviate the principal objection which ingenuity could urge against the proposition, that these vessels were the sole agents of absorption.

> " As the vessels which had been seen were generally small, and had not been traced to every part of the body, it was believed that the lacteal and lymphatic vessels were not sufficient of themselves to carry on the absorption of the body, and that they were intended only to assist the veins in the performance of this function. It became necessary, therefore, in the prosecution of the subject, to discover whether the lacteals and lymphatics were the only absorbents. The idea of their being a distinct and complete system of vessels, fully equal to manage the absorption of the body, forcibly struck the mind of Mr John Hunter, and he immediately instituted experiments to confirm or overthrow it. For this purpose he made many experiments on living animals, by throwing different fluids into the intestines for absorption; and found that the lacteals always became filled with the fluid, but never the veins. The same opinion he confirmed also by injections, so as to establish it beyond a doubt." p. 54, 55.

The accuracy and competency of these experiments have, it is well known, been recently questioned, and the truth of the whole theory of lymphatic absorption been vigorously assailed by Magendie,—and some of his pupils; and the Hunterian doctrine of absorption can no longer be regarded as the same undoubted theory, which it was considered by its proposers, and is here represented by their eminent kinsman. Magendie, however, though he has succeeded in shaking the foundation of the doctrine of Hunter, has by no means been equally fortunate in reviving the ancient one of absorption by the veins. His experiments, with those of Flandrin, and of Tiedemann and Gmelin, and more recently of Treviranus, prove what was long known, that the veins, under certain circumstances, possess the power, and exercise the process of absorption; but physiology is still destitute of a sufficiently copious and extensive collection of facts, to convert into an established doctrine the two principles for which Magendie has so strenuously contended,—that the veins absorb under all circumstances, and that they are the exclusive, or even the principal agents of absorption. It would have been interesting to know the opinion of so able an anatomist as Baillie on this question; but it is manifest that the present discourse was written at a period long previous to that at which the question was agitated, and he had not regarded the objections as sufficiently valid to induce him to give them any place of consideration.

The subject of Magnifying-glasses and the Microscope next claims attention; and the labours of Malpighi in minute anatomy, and of Leeuwenhoeck in microscopic observation, are briefly noticed. In the department of preparation and conservation, the names of Swammerdam, Van Horne, De Bils, and Staelpaart Van der Wiel, carelessly called Hallpert Vander Wiel, by error of printing, are commemorated. Under the head of injection, justice is done to the fame of De Graaf and Ruysch; and Dr Nicholls and Mr John Hunter are recorded as the inventors and improvers of the art of making corroded preparations.

The character of John Hunter is well delineated.

"His mind was bold and inventive, treading constantly in a path of its own, without regard to the common tract which had been followed by others. This was aided by an industry and enthusiasm, of which it would be difficult to find any superior example. With such singular endowments for the cultivation of science, his progress was proportionably great. There is no subject which he has considered, to which he has not added new light; and many branches of science he has very materially improved. That two brothers should have become so eminent in the same pursuits, is almost without example; and it is equally rare that two brothers should have formed collections of such magnitude for the public interests of science. These will remain to future ages as monuments of their industry and talents, and will call forth the grateful acknowledgments of posterity. By such examples may we likewise be excited to exertions that may not be unworthy the approbation of our country." p. 80.

But the part of the discourse which will be perused with most interest by the medical reader, is that in which Dr Baillie delivers his opinions on the nature of disease, and on the light which it receives from the study of anatomy, sound and morbid. Every one must be anxious to know the opinions of a physician who, to all the philosophy of the subject derived from anatomical knowledge, added whatever experience had been enabled to collect during a life of intimate acquaintance with the phenomena and varieties of unhealthy action. These opinions are not so remarkable either for novelty or striking originality, as for simplicity and perspicuity; and so natural and unaffected are they, that any reader might suppose he knew the whole of them before he read them. How valuable such views must have been to those commencing the study of medicine, can be justly comprehended by none but those who are introduced at once into all the mysteries of unintelligible theory, and all the technicalities of affected art.

"Disease may be said to be a wrong action either of the body generally, or of particular parts. Sometimes disease terminates by a gradual diminution of the diseased action, without any visible effects being left behind; while, at other times, it produces some alteration of structure, or the formation of a new substance; but these ultimately depend on diseased action.

"A disease must always have relation to a healthy action, or healthy structure of parts, for it is only a deviation from them; so that a knowledge of disease would appear to rest on a knowledge of the body in its healthy state. It is unfortunate that the peculiar mode of deviation is not always discoverable; but it is evident that we cannot, on any occasion, become well acquainted with the deviation, without having previously known the original state. It is in this point of view that anatomy and physiology become so very important, as affording the most likely means of relieving the body when suffering under disease.

"When the structure and natural action of any part is understood, it will best assist us in discovering what causes may influence the action which produces a morbid change, and how the application of other influences may probably counteract these causes, so as to protect the body from them. It will go still further; for when the causes have been applied so as to produce their effects, it will be most likely to point out what may be most serviceable in bringing back the parts to a natural mode of action.

"The symptoms of a disease are appearances which take place while the body is under a diseased action, and depend on something more secluded from observation. If they arise simply from diseased action, no other change having been produced, they leave no traces after death; and there is room for conjecture, which will be more or less reasonable, according to the understandings of different persons, and their degree of knowledge of the animal economy. But diseased action often produces a change of structure, or the formation of a new substance, or a new situation; such changes becoming themselves the cause of many symptoms, and being, as it were, intermediate between the symptoms and the diseased action. This gives the opportunity of anatomical examination, so as to trace more distinctly the nature of the disease, to establish a better reasoning upon it, to point out how we should act, what we may hope, and what we may fear.

"It is by no means uncommon that diseases, very different in themselves, should have symptoms a good deal resembling each other. These may very often be confounded, especially by superficial observers; but even men of the soundest judgment are liable to be mistaken. When, however, bodies are examined after death, and the disease is ascertained, the mind will be naturally led to attend minutely to symptoms on a future occasion, endeavouring to detect some marked difference, by which the disease may be distinguished in the living body. Thus opinions and conjectures will be corrected by facts, and physic be daily advancing upon surer grounds."

The illustrations of the necessity of anatomical knowledge, both to the physician and surgeon, are forcible and happy, and are well adapted to kindle a spirit of enthusiasm and zealous application in the breast of the pupil.

"There is nothing which renders a person so capable of discovering new diseases, as a knowledge of anatomy. Who can be so well fitted for this purpose, as he who is familiar with the natural structure, and the diseased appearances in an animal body? What person but an anatomist could have discovered the varicose aneurism, or, taking advantage of Baron Haller's observations upon the *hernia* congenita, could have explained the cause of that species of hernia in which its contents are in contact with the testicle? There are diseased appearances which are very common in an animal body, and which are of no great consequence; and it requires a familiarity with the human body to be able to distinguish them from appearances strictly natural, and from diseases that are really serious. This last circumstance is of great importance, when we consider how often we are desired to examine bodies after death for the satisfaction of friends, or are called upon to do so by the laws of our country. Upon our judgment may depend the life of a fellow-creature. What reflection can be more serious to a man of humanity? But, independently of these circumstances, there will be many cases of doubt presenting themselves to our minds, which we would wish to settle, but shall not be able, unless we are acquainted both with natural structure and diseased appearances. Indeed, how otherwise is physic to be improved?"

"Anatomy is still more closely connected with surgery than with medicine. The first being concerned in local diseases, is capable of receiving more light from it than the other, which is more employed about general diseased action. A surgeon who has taken care thoroughly to study anatomy, besides having a chance to be better acquainted with disease, and better able to distinguish between two diseases which are similar, is better fitted to act in any operation. His hand is acquainted with the knife, and he can act more certainly and surely. The knowledge of anatomy fits a surgeon for performing operations in uncommon and difficult situations, teaching him where he should cut with freedom, where with extreme caution, and where not at all. It fills his mind with resources. It points out to him where an operation may be improved, and suggests to him what steps he should take, when circumstances prevent an operation from being done in the usual manner. In a new situation, it will tell him whether he may operate or not, and what plan of operation he is to follow. In fact, anatomy is the basis of surgery, and without it a surgeon neither acts with safety, nor accommodates himself to the infinite variety of circumstances which may occur.

" If, then," he concludes, " anatomy be of so much use in physic and surgery, it ought to be earnestly cultivated by those who really wish to understand their profession, and to become respectable in it. This is not a trifling matter. Justice and humanity require every exertion where the lives of our fellow-creatures are concerned. There are many professions where negligence or inattention may be reckoned a folly, but in ours it is a crime." pp. 84–87.

These observations are certainly neither original nor uncommon, and are so obvious to every person in the least degree acquainted with the subject, that they must be unhesitatingly and unanimously admitted. But they demonstrate the pains and anxiety with which this eminent physician impressed on the minds of those entering on the study of medicine, the indispensable necessity of that knowledge, without which medical observation must be destitute of its surest and safest guide, and medical prescription and treatment may be converted into blind empiricism and downright quackery. That his exhortations might lose none of their force, he not only addresses his arguments to those feelings of honour and mental comfort which are not satisfied without adequate stores of professional knowledge, but he shows the most amiable solicitude in inspiring his auditors with confidence in that great principle by which, amidst the apparent confusion and indiscriminate arrangement of good and evil in human affairs, true merit is never suffered to go unrewarded, or to languish in heartless neglect and obscurity.

" And success, as well as character, depends on attention to education. It is an ill-founded notion arising from deserved disappointment, to say that merit is neglected. It is sometimes joined with circumstances that may have some influence in counteracting it, as for instance an unfortunate temper, or a disagreeable manner; but generally it meets with its due reward. The world does not often judge erroneously; persons of merit have at least the best chance of success; and who would be ambitious of the public approbation, if it had not the power of discriminating?" p. 91.

These sentiments are highly honourable to the individual who delivers them; and coming from one whose brilliant success had been the immediate effect of mere personal merit, add one to the many testimonies of the general justice with which prosperity and adversity are eventually found to correspond to the actual merits of the candidates.

The second part of the volume, consisting of the three Gulstonian Lectures, is somewhat different; and, as was to be expected from the situation in which they were read, and the subject of which they treat, their general tone is more strictly philosophical, and their whole character more exalted and dignified. Their subject is the nervous system; and the first lecture is devoted to a succinct and clear exposition of its general anatomical characters.

On this subject, at once obscure and extremely curious, which has been investigated with so much diligence and research, it was not to be expected that much information either of novelty or of great interest could be communicated in the compass of a single discourse; and it is chiefly to the circumstance of seeing in what manner the mind of Baillie would treat a subject, which occupied the study of some of the most able philosophers of whom the science of medicine could boast, that the curiosity of the reader will be attracted. Without attempting, therefore, a full analysis, we select only a few topics for observation.

" The *dura mater* consists of fasciculi of white fibres compacted into a firm membrane. These are more readily observable upon the outside of the *dura mater* than on its inside, because they are coarser. The internal surface of the *dura mater* is finer, and of a more shining colour than the external; and here the fibrous structure is less observable, except in particular situations, where coarse fasciculi are interwoven with the membrane, in order to give it greater strength. The *dura mater* may, in some degree, be considered as consisting of two laminæ,—a coarser upon the outside, and a finer upon the inside; but these cannot be separated from each other so as to leave natural surfaces. A number of fibres are always seen passing between the one and the other, by which they are united."

This description, which is clear and pointed, is correct so far as it goes; but, if we may be permitted to offer an opinion on the representations of an anatomist so careful and so experienced as Dr Baillie, we should say that it fails in not being carried to the proper length. We allude particularly to the view which he gives of the inner fine lamina, which is undoubtedly to be regarded as the outer or cranial division of the arachnoid membrane. This may be proved in a variety of modes; but is perhaps most easily understood by tracing the *dura mater* from those points at which it adheres to the inner surface of the cranium, to those at which the several nerves issue from that cavity by the cranial holes. Thus, if the optic nerves be attentively examined at the spot where they enter the optic *foramina*, the *dura mater* which covers the bone surrounding these holes will be found to go a very short distance into them, to stop suddenly and be *reflected* backwards, as anatomists speak, to cover the nervous trunks, and thence to extend along the *pia mater*. Now the *reflected portion* here, or the *fine inner lamina* of Dr Baillie, is in truth the arachnoid membrane which lines the *dura mater*, passing from it along the nervous chords to the outer surface of the *pia mater*. In the same manner, if the third pair or oculo-muscular, or fifth or tergeminal nerves be examined at the openings at which they pass through the scull, the *dura mater* is found adhering firmly round their several margins to the bone by its outer or attached surface, while its inner or free surface turns back on the nervous chords, and thence is continued over the *pia mater*. In short, these nerves are covered with arachnoid membrane only (always excepting the optic, which has a firm *neurilema*, generally considered, though on insufficient grounds, as *dura mater*); and while the *dura mater* is proper to the inner surface of the scull, and the *pia mater* to the outer or convoluted surface of the brain, the arachnoid membrane is common to both, and invests not only the outer surface of the *pia mater*, as is commonly taught, but the inner surface of the *dura mater*.

Another proof of a similar description of this arrangement is derived from examination of the free or lower margins of the two dichotomous membranes, the *falx major et minor*, and of the transverse portion of the *dura mater* or *tentorium cerebelli*. In the first place, at the upper or convex margin of the great *falx*, where the veins pass from the *pia mater* into the longitudinal sinus, it is not difficult to trace the arachnoid membrane from the pia mater along their coats, to that portion of the dura mater which forms the sinus, and *vice versa*, or the inner lamina of Dr Baillie, from the falx, along the veins to the outter surface of the pia mater. In the second place, we find the lower or concave margin of the falx, connected at the bottom of the middle fissure between the hemispheres, to the pia mater on either side by thin, transparent, filamentous membrane, which is nothing but the arachnoid passing from the falciform process of the *dura mater* to the outer surface of the *pia* *mater*, where it covers the commutual surfaces[*] of the hemispheres; and the same may be seen at the bottom of the small or cerebellic falciform process. In the third place, at the inner margin of the transverse portion or tentorium, the inner lamina of the *dura mater* may be traced passing off to the *pia mater* of the brain above, and of the cerebellum below.[†]

These observations will perhaps not be without their use in preparing the reader for Dr Baillie's account of the *pia mater*.

"The pia mater lies in immediate contact with the substance of the brain. It is a very fine transparent membrane, upon which are spread a great number of arteries and veins. These contain partly a florid and partly a dark-coloured blood, so as to produce a varied appearance; and there are few objects more beautiful than the pia mater crowded with blood-vessels. In most parts there is no adhesion between the pia mater and the *dura mater*; but in some they are connected as in the neighbourhood of the larger sinuses of the *dura mater*, into which the veins of the pia mater pour their contents." (We have explained the light in which this connexion is to be viewed.) "To the internal surface of the pia mater there are attached a great number of processes, which consist of a shaggy membrane crowded with very fine vessels. The vessels do not indiscriminately enter every part of the substance of the brain; but in the cerebrum they enter between its foldings, which have been named convolutions; and in the cerebellum they enter between its concentric laminæ."

This is undoubtedly the common view of the *pia mater* given by anatomists; but it is incomplete and deficient, if not erroneous. The pia mater is, as we have already said, the *proper membrane* of the brain, and consists of two parts, an outer covering the *convoluted surface* of the brain, and an inner entering into the cavities formed by the inner or *figurate surface*, and spread over this surface in the form of what has been termed the *plexus choroeides*. That these two divisions are continuous with each other, and form parts of the same membrane, may be shown by tracing the pia mater first behind and below the mesolobe or middle band (*corpus callosum*), where it is continuous with the *velum interpositum*; secondly, from this point into the lateral ventricles in the form of choroid plexus; thirdly, into the posterior-inferior *cornu*, where it covers the great *hippocampus*; and, fourthly, at the angle between the cerebellum and *medulla oblongata*, where it forms a very minute choroid plexus, seldom noticed by anatomists. Each choroid plexus, or each division, to speak more accurately, of the choroid plexus, is connected by means of minute blood-vessels to the figurate surface on which it rests, and it appears to perform the same function of sustaining vessels as the *pia mater* does to the convoluted surface.

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[*] For the explanation of this term we refer to a former article in volume XXI., p. 98 and 12.

[†] We speak in reference to the situation of parts in the human subject.

If it be objected to this view of the choroid plexus, that it has not the appearance of a membrane, this point is very speedily settled by immersing it in water, when, with a little management of the probe and forceps, it can be spread out exactly like the *pia mater*, which it closely resembles. It is this membrane which is principally affected during the morbid process which terminates in watery effusion (*hydrocephalus internus*), in the cephalic form of continued fever, in the serous apoplexy of authors, and wherever watery fluid is found in the lateral ventricles of the brain. It is further not unworthy of remark, that when this membrane secretes fluid in the figurate surface of the brain, the pia mater forms a similar fluid at its outer surface, in a most delicate cellular tissue between it and the arachnoid membrane, to which it then gives the appearance of being raised by a thin jelly spread under it. "When a fluid," says Dr Baillie, "resembling thin jelly, is thrown out under this membrane (the arachnoid), as frequently happens during fevers, it may be seen in many parts separated from the pia mater, and is then somewhat thickened." The thickening is not real, but only apparent, and arises from the distention of the cellular tissue, which connects it to the pia mater.

The observations on the brain proper and its parts, and on the relative proportion of cortical and medullary, or brown and white matter, are short and hasty, but sufficiently pertinent. Dr Baillie justly denies the vulgar doctrine that the cortical matter is a congeries of minute vessels, and most of the other notions entertained by different anatomists on this little known species of animal matter.

"Of the intimate structure of the cortical substance I believe very little is satisfactorily known. Some have thought it to consist of nothing else than a congeries of very fine blood-vessels. It is evident, however, that no mere collection of vessels could be made to resemble the cortical substance. The pulpiness so remarkable in that substance could not be produced by a simple collection of vessels, however minute; nor can one imagine a collection of vessels to have the same properties with any part of the brain. When a part of the cortical substance is injected with fine injection, it appears to be very plentifully supplied with small blood-vessels; but still it is very far from being entirely composed of them."

On the results of microscopical observation, we have the following remarks.

"Some modern anatomists and naturalists have examined the cortical substance with microscopes of very great magnifying powers, but the result of their observations has been different,—a strong proof that much dependence is not to be placed on their accuracy. Some have thought it to consist of very small rounded bodies placed in a transparent jelly; others have thought it to consist of rounded bodies placed in a fine cellular structure; and others have supposed that it consists of very fine tortuous tubes, with some small rounded bodies intermixed. With regard to myself, I cannot trust my own accuracy in making minute microscopical observations; and I frankly own that I am totally unacquainted with the minute structure of this substance." p. 110.

Though absorbents have not been demonstrated in the brain, Dr Baillie entertains very confident hopes of their subsequent discovery.

"There is every reason to suppose that the brain has absorbent vessels as well as arteries and veins; but they cannot be very obvious or they must have been long ago ascertained. The nature of the substance of the brain must add very much to the difficulty of their detection; but time, perseverance, and favourable opportunities will at length overcome every difficulty, and the absorbent vessels of the brain will be as certainly known to anatomists as those of other parts. Many circumstances show that absorption takes place in the brain, and it cannot be believed that it is carried on by a different apparatus, or on different principles, than in other parts of the body."

This is extremely dubious; and whether absorption takes place in the brain or not, the force of analogical reasoning is not great to show that the same agents and same means are employed here as in other parts to produce the effect.

The subsequent part of the discourse is occupied with the exposition of the structure of the spinal chord and its membranes, and of the nerves, their ganglions and plexuses; but contains nothing on which we could with propriety make any observations.

In the two next lectures the functions of the nervous system are discussed. A pretty full and correct view of the physiology of this system, as it was taught by Haller, Whyte, and Cullen, is given with great neatness and precision; but we shall not enter into any details, as it is impossible to communicate any adequate idea of it by mere excerpts, and it would be difficult to select any individual passage more than another.

We proceed, therefore, to the third part of the volume, which is unquestionably the most valuable, though the least imposing, and in which we doubt not the medical reader will easily trace the strong sense and judicious observation which peculiarily distinguished the character of Dr Baillie. It consists of short observations chiefly on the treatment of diseases derived from the personal experience of thirty years. Dr Baillie appears to have commenced the plan of recording the results of his experience in the year 1819 only, when he was residing at Hanger Hill near Acton, during his attendance at Windsor. It is to be regretted that he was prevented by the incessant and fatiguing duties of his profession from making these records more copious, and more extensive, as the specimen before us demonstrates how valuable the results of his extensive experience must have been. Whatever aid we derive from anatomical knowledge in *distinguishing* diseases and unfolding their nature, it is chiefly to experience that we look for the means of treating them successfully. It is this circumstance which gives the intrinsic value to the results of observation and experience, and which renders long experience more valuable than short, and judicious observation more valuable than that which is desultory and careless. Yet it is not always that the results of experience can be embodied into general rules; for so many minute circumstances occur in almost every individual case, that nothing requires a more delicate and nice exertion of the faculties than the application of remedies to the peculiarities of individual cases; and whoever excels most in this faculty, will also be the most successful practitioner of the art of healing. How thoroughly Dr Baillie understood this subject, will easily appear from the following remarks.

"I am convinced," says Dr Baillie, in the beginning of these observations, "that the most successful treatment of patients will depend upon the exertion of sagacity or good common sense, guided by a competent professional knowledge; and not by following strictly the rules of practice laid down in books, even by men of the greatest talents and experience. It is very seldom that diseases are found pure and unmixed, as they are commonly described by authors; and there is almost an endless variety of constitutions. The treatment must be adapted to this mixture and variety, in order to be as successful as circumstances will permit; and this allows of a very wide field for the exercise of good common sense on the part of the physician. A physician who should be guided strictly by the rules laid down in books would be a very bad practitioner. In the following short observations on the treatment of various diseases, I shall state impartially the result of my experience, without entering into any speculative reasoning which is often very fallacious." (p. 163.)

The diseases which come under review in this manner, are

Complaints of the Head.
Some Diseases of the Neck.
Some Diseases of the Chest.
Palpitation of the Heart.

Diseases in the cavity of the abdomen.
Inflammation of the Bowels.
Some affections of the Liver.
Some diseases of the Pancreas.
Some diseases of the Spleen.
Some affections of the Kidneys.
Some diseased affections of the Urinary bladder.
Some diseased affections of the Womb.
Some diseased affections of the Ovarium.
Fevers.

COMPLAINTS OF THE HEAD. *Sick Headach*, or that which depends on the state of the stomach and bowels, or an irritable state of some of the nerves of the scalp, is not benefited by bloodletting either general or topical. The remedies are temperate living, great attention to avoid improper diet, bitters and purgative medicines. The best common medicine is rhubarb and soap, in such doses as to give two motions daily. Exercise, especially on horseback, is useful; and keeping the hair very short, so as to allow the application of cold by a large sponge wrung out of water, gives temporary relief. In some instances where all remedies are unavailing, these complaints disappear gradually after some months or years.

Apoplexy. Dr Baillie knew one instance of fatal apoplexy where many of the blood-vessels were found after death to be much distended with blood, but without extravasation in any part of the brain. Besides large bleeding to be repeated according to circumstances, topical bleeding is of use. If the patient escape by the ordinary means, it is safest to live almost entirely throughout future life on vegetable food, to abstain from wine, spirits, and malt liquor, and to shun strong or continued exertion of the mind. In a few instances after the fullness of the vessels of the brain had for some time subsided Dr B. has derived advantage from the moderate use of tonic medicines, especially steel.

Hydrocephalus. "I have known in my own experience but one instance of this disease being cured when fully formed. In this case all the symptoms were well-marked, and the disease had made such progress that squinting and an irregular pulse had taken place. There had been no peculiar treatment, except that mercurial ointment was applied daily to a considerable sore on the upper part of the head, which had been produced there by a blister. The individual is now alive, and is a young lady of good talents, which she has highly cultivated."

Epilepsy appears to Dr B. to have become much more frequent within the last twenty years than formerly. He has known very few instances radically cured, but a considerable number in which the intervals between the fits were rendered much longer. The medicines most useful in contributing to this effect were misletoe of the oak, oil of amber, and lunar caustic, the last of which is most powerful; but in two instances it tinged the skin of a dark colour. The bowels should be kept open, and brisk purgatives are useful in the commencement. Temperance in the use of animal food, total abstinence from wine, ale, and porter, shaving the hair and cold applications are inculcated.

Tic Douloureux seems also to have become more common of late years, and more frequent among men than women. Dr B. does not recollect of having seen any instances of permanent cure either by internal medicines or by operation. Temporary cures for some months have been effected under the use of Peruvian bark and of arsenic. "The operation of dividing the nerve has in some instances prevented a return of the disease for one or two years, but has not, as far as I know, prevented it permanently."

DISEASES OF THE NECK.—*Swelling of the Lymphatic Glands* is tedious, and not much benefited by medicine. The most useful remedies are sarsaparilla with soda, Peruvian bark with soda, and some form of steel. Sea-air and tepid sea-water bathing are often beneficial; but the air and waters of Malvern are more useful than any other remedy.

Bronchocele is not very uncommon in this country, more frequent among women than men, and among young than old persons. Not much under the influence of medicine, it may frequently disappear spontaneously. Dr B. knew one or two cases in which the patient was destroyed by the swelling compressing the windpipe and œsophagus. The most useful remedies are burnt sponge, soda, and mercury used externally, either as ointment or plaster.

Chronic Inflammation of the Larynx and Trachea, occurs frequently, and is more common among men than women. Often confined to the inner membrane of the larynx and upper part of the trachea, it frequently spreads downwards and may affect the inner membrane of the bronchia, continuing several months, or for years with short intervals of amendment, and not unfrequently laying the foundation of consumption. The influence of remedies is either very gradual or not at all; benefit may be derived from the repeated application of leeches at short intervals to the fore part of the neck, or the skin covering the upper bone of the *sternum*, and from blisters; but perhaps the most useful remedy is a small seton inserted under the skin of the side of the neck very near the larynx. Internal medecines have little effect; but the most useful is extract of hemlock in doses of five grains, three times daily for some weeks.

Quinsy.—The attempts to prevent suppuration by applying leeches under the angle of the lower jaw, have generally no other effect than that of retarding inflammation, and protracting the sufferings of the patient. If after one or two applications of leeches, the swelling of the uvula and tonsils is not materially lessened, the vapour of warm water is to be inhaled and poultices are to be applied to the external fauces.

DISEASES OF THE CHEST, *Pleurisy and Peripneumony.*—When the great remedy, bloodletting, has not been applied early enough, or in sufficient quantity, and an abscess has been formed in the lungs, which has burst, patients have, in the greater number of instances seen by Dr Baillie, recovered but very slowly. Under these circumstances, the medical attendant has little to do but avoid mischief. The constitution should be moderately supported without being too much stimulated. Moderate doses of myrrh, decoction of bark, or infusion of some bitter, with light animal diet, and even a little wine, are sometimes of use; but care must be taken not to excite new inflammation.

"In the course of my experience through many years, I have known a few instances of abscess being formed in the lungs without any previous pain in the chest, or difficulty of breathing or observable fever. Such patients upon some exertion, or even without exertion, have suddenly coughed up a considerable quantity, perhaps half a pint or more of pus; and this has been to the patient the first intimation of disease. In such cases the inflammation of the lungs must have proceeded so slowly as to have produced little or no pain in the chest, and not to have alarmed the constitution so as to excite fever."

It is probable that these cases were the chronic pleurisy so well described by Baglivi.

Consumption.—Dr Baillie knew one or two cases of patients who recovered from *phthisis* apparently fully formed. He thinks however he might have been mistaken, and further inquiry would have shown that they were small absesses of the lungs of a common and not a strumous nature. He had known a good many instances in which persons threatened with consumption had recovered by going into a milder climate, or even into Devonshire or Cornwall; but he does not recollect a single instance of recovery when the disease had been decidedly formed. On the advantages of particular places, he conjectures that different situations may suit better with the constitutions of different individuals. When there is no symptom of active pectoral inflammation, white fish or light animal food may be eaten at dinner; and he has occasionally seen benefit from taking one or two glasses of wine diluted with water; but wine is generally improper. He has seen a good deal of temporary advantage from myrrh, from ammonia, and from light bitters united to the acetic acid; but no medicine is of permanent and substantial use. Repeated blisters, or a seton inserted under the skin, in some part of the chest, are occasionally beneficial.

Hydrothorax, when unconnected with disease of the heart or lungs, is generally more readily affected by medicine than ascites or ovarian dropsy. The most efficacious medicine is mercury combined with squill and foxglove, in the proportion of five grains of the blue pill, with one grain of the powder of squill, and half a grain of that of foxglove given twice or thrice daily. Dr Baillie, however, does not remember one instance of permanent cure, though he has seen a good many in which the symptoms were repeatedly removed by the same means in the same patient. He speaks favourably of scarifying and cupping the ankle as a means of relieving difficult breathing.

Palpitation of the heart may take place at any period of life, but is more common at an early period, as from fifteen to twenty-five years of age, when it depends either on morbid irritability of the nerves of the heart, or on some imperfect state of digestion. Rest, temperance, and shunning mental irritation, are the most effectual remedies in the first case; and, in the second, the condition of the stomach must be improved, and constipation obviated.

Of the palpitation which depends on enlargement of the heart from rheumatism, he speaks in the following terms.

"The enlargement, in general, goes on increasing till life is destroyed; but I have known two cases where the enlargement stopped at a certain point, the increased action of the heart in a great measure subsided, and the patients acquired a tolerable degree of health. *They are both now alive*, and they have the prospect of living, with care, till the ordinary term of life. Such a fortunate issue is rare; but the disease may be generally retarded in its progress by much rest of body, quietness of mind, and a very temperate mode of living."

Angina pectoris he represents as almost constantly depending on ossification of the coronary arteries; but he admits that he met with two cases in which symptoms exactly resembling those of this disease depended upon imperfect digestion, and disappeared as the stomach improved.

DISEASES IN THE CAVITY OF THE ABDOMEN. *Ascites*, depending on morbid state of the liver or spleen, is never permanently removed; and even when the viscera are sound, it is, according to the experience of Dr B., rarely cured by medicine

The most efficacious remedies are supertartrate of potass, and small doses of elaterium. " In two or perhaps three cases during my experience, ascites has gradually got well without medicine, after the common remedies had been sufficiently tried, and had failed. " Of the efficacy of bloodletting, he entertains no doubt from the testimony of late publications; but he has not sufficient personal experience of this remedy to enable him to appreciate its value.

Peritonitis has been more relieved by repeated application of leeches than by general bleeding. The most efficacious purgatives in this disease are calomel and the neutral salts.

Affections of the Stomach. The most important is one in which the stomach throws up in large quantity a fluid like cocoa—a quart at a time, and this repeatedly for many days together. He regards this condition of the stomach as sometimes connected with a diseased state of the liver. It proved fatal in several instances;—but in others, especially in two cases, the complaint subsided for several months at a time, and the persons enjoyed, in the interval, tolerable health, and are still alive. " In one case I had an opportunity of examining the condition of the stomach after death. It was very capacious, and was half filled with this brown fluid, but did not appear to be at all diseased in its structure. The neighbouring viscera, as the liver and spleen, were perfectly sound. The fluid would appear to be formed by a diseased secretion of the inner membrane of the stomach, without any apparent morbid structure." This disease is little under the influence of medicine or diet; but, in two or three cases, benefit seemed to result from astringent medicines combined with moderate doses of opium, and from obviating costiveness.

In simple atony of the stomach, the remedy is a course of the Bath waters.

Dysentery should not be treated with opiates and astringents till the stools are freed from mucus and blood by the use of mild purgatives, especially castor oil. " As there is always an inflammatory condition of the bowels in this disease, leeches may be applied to the seat of the sigmoid flexure of the colon, and the upper extremity of the rectum, with a considerable chance of benefit. "

In defective secretion of bile, even when this fluid is unusally dark coloured, he speaks highly of the blue pill and purgatives continued for some time; but disapproves of long or liberal courses of mercury. " When mercury is carried beyond the point that is necessary, it often injures the constitution by weakening it, and by rendering the nervous system very irritable. "

Abscess of the Liver may break externally, when the patient recovers entirely, unless there be something unfavourable in the constitution. When it communicates with the lungs, and the matter is brought up by coughing, the patient, if prudent, and possessed of a tolerably good constitution, will sometimes at last entirely recover. When it communicates with the stomach, and the matter is discharged sometimes by vomiting, sometimes by the bowels, he will also not unfrequently recover. And though the same observation may be extended to an abscess of the liver, which communicates both with the stomach and the lungs, the circumstances are more unfavourable in this than in the other two cases. In these cases medicine can do little benefit; but much injury may be done by imprudent or unskilful management. Stimulating diet, too much or too violent exercise, and exposure to cold may lead to a fatal event.

The *Pancreas* is less liable to disease than any other important gland in the body. Dr Baillie has known only a solitary example during the thirteen years in which he was physician of St George's Hospital, under the care of Dr Heberden junior; and this was not known till after death. This circumstance would prove rather the little effect disease of the *Pancreas* exercises on the functions, than the absolute rarity of the occurrence,—were it not that Dr Baillie expressly states, that of all the dead bodies which he had examined, he had not found a single instance in which the pancreas was so large that it could have been recognized by examination during life. On the Spleen we see nothing worthy of notice.

AFFECTIONS OF THE KIDNEYS,—are more common than in most other glands, and more frequent in men than in women. *Abcesses* may be common or strumous; they are rarely permanently cured; but patients recover their general health, and retain it for several months, or even years. Medicine has little control over them. A seton inserted in the loins, or in the flank of that side where the diseased kidney is situated, is sometimes useful. Among internal medicines, the whortleberry (*arbutus uva ursi*), and the compound tincture of benzoin, have sometimes been serviceable.

Hemorrhage from the kidneys, may arise from distension of the renal blood-vessels, and consequent rupture, or from the laceration of a sharp concretion. The remedies are bleeding, general and topical especially, cold applications to the loins and belly; as internal medicines, great benefit has often been derived from nitre, the dilute sulphuric acid, and the tincture of muriated iron,—the last of which is particularly useful; and, lastly, quiet, a cool chamber, and vegetable diet, are indispensible conditions to a favourable termination.

"I do not recollect any instance in which the patient has not recovered from this complaint, when it has not been connected with abscess, or some other formidable disease of the kidneys."

Diabetes.—Among a good many instances, a considerable proportion have got well, or had their symptoms very much mitigated, under the care of Dr Baillie. The most successful plan of treatment, was to give considerable doses of opium, combined with rhubarb, or some other bitter. Bleeding from the system, and topical bleeding from the loins, are often useful. The diet should be temperate, but chiefly of animal food; and the best drink is Bristol water.

In the observations on the DISEASED AFFECTIONS of the Urinary Bladder, Womb, and Ovarium, there is nothing which requires very particular notice.

OF MY EXPERIENCE IN FEVERS.—In *Typhous Fever*, the experience of Dr Baillie was derived chiefly from the cases of St George's Hospital, during thirteen years attendance as physician. The contagious property of these fevers he regards as inconsiderable, but entertains no doubt of their being readily communicated from one individual to another, when patients are crowded together, and the apartments are ill ventilated. He admits of no remedy having specific powers in checking the disease; and he regards the most successful treatment to be that which removes or mitigates symptoms as they rise. Local affections of the head, chest, or belly, must be controlled, in the first case by cupping, leeches, and cold applications; in the second, by cupping, leeches, blisters, and saline medicines; and in the third, by purgatives, leeches, and fomentations or blisters. Too vigorous a circulation over the body, without apparent local affection, may be corrected by a very cautious bleeding from the arm, by purging, and by saline medicines; but a constitution with feeble actions is to be strengthened with stimulating remedies, especially wine in suitable doses. The following observation on blood-letting deserves notice.

"During the greater part of the time in which I have practised medicine, physicians in general, and myself among that number, have, I believe, been too sparing in taking away blood in typhous fever. It was hardly ever directed to be taken away from the arm, and not often locally, except by the application of leeches to the head. Of late years, many physicians have gone into the opposite extreme, and have taken away blood too profusely. In the course of a few years, this remedy, like every other, will find its true level."

Intermittent Fevers, it is well known, are less common in London than formerly. Dr Baillie, while he was physician to St George's Hospital, saw perhaps five or six cases annually, chiefly among the poor Irish lodged in St Giles's. In some of these cases, the origin of the disease could be clearly traced to marshy effluvia; but in others not, as the patients reported that they had lived in St Giles's for several years, and had always been employed as labourers in London. Dr Baillie, however, admits that these persons may have been exposed to marshy effluvia in the neighbourhood, without knowing or recollecting it. A similar remark had been previously made by Willan and by Bateman.

Dr Baillie has known a good many cases in which bark alone would not cure ague; but when a grain of calomel was given every night for eight or ten evenings, bark cured the ague in the course of a few days. Arsenic cures ague in general sooner than bark, and produces no bad effect, if it be given in proper doses, and be not continued too long. When the ague has been stopped for three or four days, Dr Baillie recommends the arsenic to be given up, and half a drachm of bark in powder to be administered three or four times a day for perhaps a week. Some cases he has seen cured by the powder of *Calamus Aromaticus*, or Sweet Flag, which is not an uncommon remedy among the lower orders of people in Sussex.

We have been more full and minute in our account of the contents of this volume, because it is printed, but not published, a distinction which we think should not have been made. Our readers, we doubt not, will agree with us in the opinion already expressed of their value, especially of the Observations. We conclude with remarking, that it is unfortunate that the volume was not given to the public along with those of Mr Wardrop, which would have rendered the observations of their illustrious author much more extensively useful to the profession, than their present condition allows them to be.

V.

The Study of Medicine. Second Edition. By JOHN MASON GOOD, M. D. F. R. S. F. R. S. L. Mem. Am. Phil. Soc. and F. L. S. of Philadelphia.—In Five Volumes. London, Printed for Baldwin, Cradock, & Joy, 1825.

OF this learned and elaborate work, the present edition is both enlarged and improved. The science of medicine, we are happy to think, is stationary neither in its principles nor in its practical applications; for as the former are undergoing extension and correction, the latter are rendered more simple, more powerful, and more direct. The first edition of this work exhibited a very comprehensive view of the state of medical knowledge at the time of its publication three years ago; but what was correct and complete in 1822, might require modification and addition in 1825; and with this laudable object the learned author has availed himself of the demand for a new edition of his work, by introducing such changes and supplementary matter as the progressive improvement of science has since suggested.

The general arrangement and substantial contents of the work remain indeed the same; but many articles have received important additions and corrections; several have been new modelled and almost written over again; in some part of the work chapters entirely new have been introduced on diseases which previously were either imperfectly or erroneously described, or entirely omitted; and in every part of the work great pains have been taken to render it as complete a body of medical information as possible.

Of these improvements the sources are various. Dr Good has not disdained to illustrate British pathology and practice, by reflecting on it some of the clearest and steadiest rays of those lights which adorn the medical schools of France and Germany; and he has been studious to obviate the objection which was perhaps not unreasonably advanced against the first edition of his work, that he had taken too little notice of the contemporaneous medical writers of the Continent. So numerous, however, are the publications both periodical and occasional of foreign countries, so multifarious are their contents, and so various their merits, that it is no light task to make the proper selection and concentration. In keeping within reasonable bounds, Dr Good has exercised much judgment and discretion in giving what is valuable, without what is immaterial or indifferent, and in adhering rather to the useful and necessary than to the curious. We know not whether his labours in this respect will obtain the applause of the exclusive admirers of foreign medicine and pathology; but they merit at least the approbation of the diligent student and the rational physician.

The experiments of Rolando, Flourens, and Magendie in France, and of Charles Bell and Mr Shaw in this country, on the functions and influence of the nervous system, and various other physiological researches, have called for particular notice in the physiological introductions.

Dr Good, however, has also availed himself of domestic sources of information. The reports which, for several years, the Director-General of the Army Medical Board, has been collecting from almost every quarter of the globe, on every subject related with the formation, the nature, and the treatment of diseases, form a repository of important facts, to which it would be difficult to find any thing equal either in the collections of nations or of individuals. To these stores, the author has had recourse for information on various distempers of hot climates and remote countries. Thus he has been enabled to insert some recent information on Asiatic or spasmodic cholera; remarks on the hebetude of syphilitic action in the West Indies, and the comparative insusceptibility of the constitution to its power in these regions; sundry observations on the advantage of encampments in preventing the extension of yellow fever; and the account of the progress and general nature of that multiform disorder of the East, the BERIBERI.

In speaking of dysentery Dr Good has introduced some important information on the pathology and treatment of this disease, for which he makes his acknowledgements to the valuable contributions of Dr Cheyne in the Dublin Hospital Reports. On the subject of smallpox and its modifications, he has availed himself of the learned and laborious researches of Dr Thomson, and of the interesting posthumous sketch of the nosologist of cutaneous diseases. On the still undetermined question of the nature of syphiloid maladies, he has entered into more full and lengthened discussion; and he has surveyed with much care and judgment the circumstances which our recent experience has communicated on the sanative powers of mercury in pox, and on the question of the spontaneous sanability of this variable disease. Some new matter is also added on certain diseases of the organ of vision and on their respective origin from nervous or organic disorder. And the interest which the question of the contagious nature of plague recently excited both in the House and out of doors, has led him to enter more at large into the history, laws, and natural limits of this formidable malady.

Of subjects on which entire new articles have been written, the following may be mentioned:—1*st*, The ERYTHEMA ANATOMICUM, or spreading and destructive inflammation of the cellular membrane which in certain circumstances ensues on a wound, puncture, or a bruise of the hand; 2*nd*, That little known disease termed ANAEMIA or *Bloodlessness*; of which the author has given a good account from the observations of Professor Halle of Paris, and Dr Combe of Leith; 3*rd*, The MELANOSIS of M. Breschet and other authors; 4*th*, Lateral distortion or curvature of the spine, ENTASIA RACHYBIA, of which so much has recently been said by Shaw, Dods, and Bampfield; 5*th*, Pulpy disorganization of the brain, *Ramollissement* of the French pathologists, MOLLITIES CEREBRI of Rochoux, and 6*th*, A peculiar species of TRICHOSIS; T. SENSITIVA, noticed some time ago in this Journal. To these may be added a succinct account of the stethoscope and its uses in mediate auscultation, and occasional notices of the new and active forms of medicines, for which we are principally indebted to the refined chemistry of the French pharmacologists.

By the introduction of such additional matter as this, it may be supposed that the work is somewhat enlarged. The second volume, which was formerly bulky, is divided into two; and the diseases of the sanguineous function, instead of being confined as formerly to this volume, occupy in the present edition the second and third. With the other additions and enlargements in the subsequent part of the work, what was originally contained within the compass of four volumes, is now expanded into five.

Of the merits of this work we formerly expressed our opinion at considerable length; and it is not now requisite to resume the subject. Its good and bad points we canvassed in the spirit of liberal criticism, but we trust without asperity. Though we still entertain the same opinion of its defects, we must confess, that the oftener we read it, the more excellent it appears. The information is copious, accurate, and various; the research and learning unrivalled; the style clear and precise; and the language, when not too affected, is classical and pleasing. It certainly contains the most comprehensive and correct view of medical knowledge extant; and we know no work from which the student will derive greater information, and obtain it in a more interesting manner.

VI.

1. *Ueber das Alter des Bauch-und Gebärmutter-Schnitts an Lebenden.* Von Dr Mansfeld, practischem Arzte und Geburtshelfer zu Braunschweig. pp. 24. 1824.

On the Antiquity of Gastrotomy and Hysterotomy (Cæsarean Operation) on the Living Subject. By Dr Mansfeld, Accoucheur. Brunswick, 1824.

2. *Geschichte einer glücklichen Entbindung durch den Kaiserschnitt nebst Bemerkungen uber diese Entbindung's weise ueberhaupt.* Von Dr J. H. Schenk, Siegen. pp. 150. (Siebold's Journal für Geburtsulfe. Nov. 1825. Auch besonders gedruckt).

Case of Successful delivery by the Cæsarean Operation with Remarks on this Mode of Delivery in General. By Dr J. H. Schenk, Siegen, (First published in Siebold's Journal for Midwifery, and afterwards in a separate Pamphlet). Nov. 1825.

In the former of these pamphlets, Dr Mansfeld endeavours to show, that the Cæsarean operation was undertaken long before the time usually stated by different writers on this subject. Some authors conceive the case recited by Nicolaii de Falconiis [*] as the first in which the operation was performed on the living subject. Others, on the contrary, among whom is the late Osiander, [†] are of opinion, that the true Cæsarean operation was performed, for the first time, in the beginning of the 16th century, by Jacob Rufer, [‡] on his own wife; and Kurt Sprengel [§] is inclined to think, that we cannot with certainty go any farther back than the beginning of the 17th century (1610), when, according to him, gastro-hysterotomy was undertaken for the first time by Jeremias Trautmann. Some again, as Deleurge, Levret, and Mauriceau, pass over altogether the antiquity of this operation on the living subject, or like Plenk or Gardien (Dictionnaire des Sciences Medicales) amuse themselves with the testimony of Pliny, [||] who speaks of the operation only on those who die in a state of pregnancy. It is however not improbable, that about the time, or perhaps a little later, when the Cæsarian operation was directed by law to be performed on deceased pregnant persons, its performance on the living subject, owing to a series of unfortunate cases, was neglected, but some centuries after again brought into practice. It is certainly extremely probable, that during a space of more than two thousand years, this operation, which the ancients under other circumstances knew so well how to perform, was resorted to in cases of difficult labour, when the ordinary means failed in affording relief.

The assertions of Ambrose Paré, [¶] the most celebrated surgeon in France during the 16th, and of J. S. Saccombe [**] towards the end of the 18th century, respecting the Cæsarean operation, are not deserving of much attention, since **Paré** is rather inconsistent, when on the one hand he denies the possibility of the fortunate termination of this operation, (as according to him the patient must bleed to death), and then immediately confirms the numerous successful cases related by **Rousset** in his *Traité de l'hysterotomie* (Paris 1581), under the testimony of Professor de **Monanteil** by the words, *j'atteste ce que dessus.* The absurd doctrine of **Saccombe** against the Cæsarean operation, arose entirely from jealousy towards those of his contemporaries who had performed it; and in proof of this, we need only state the motive which **Saccombe** ascribes to **Rousset** for recommending this operation. "**Francis Rousset**, en propageant une operation toujours mortelle, eut le double motif du fanatisme et de l'ambition, en faisant egorger les femmes des Protestans, pour plaire à Medicis, qui en reconnoissance des ses bons offices, le fit medecin de sa cour." (*Vide Elemens de la Science des Accouchemens, par Saccombe, p.* 283.) After this, **Saccombe**'s opinion respecting the cases mentioned by **Rousset** will not be regarded with much attention. Whether, however, it be true or not, according to **Rousset**, that Jacob Rufer really performed the operation on his wife, is of little consequence, if it can be proved that, in the beginning of the eleventh century at least, if not earlier, gastro-hysterotomy was performed on the living subject.

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[*] Vide Observationes Chirurgicæ. Venet. 1491, fol.; etiam Halleri Bibl. Chirurg. vol. i. p. .

[†] Handbuch der Entbindungskunst, 2 Bd. 2 Abtheil. Tubingen, 1821 p. 294.

[‡] Fœtus vivi ex matre viva sine alterius vitæ periculo cæsura a Francisco Roussetо conscripta, Caspabo Bauhino latine reddita, variis historiis aucta et confirmata. Basil, 1591. 8. p. 117.

[§] Geschichte der Chirurgie, I. Theil, Halle 1805, p. 388.

[||] Historia Naturalis, Lib. vii. cap. 7.

[¶] De hominis generatione Cap. XXXI. "Caeterum non possum satis mirar eos qui sibi visas mulieres affirmant, quibus non semel novacula abdomen cum subjecto utero rescissum sit ad foetum, numquam alioqui proditurum, extrahendum. Id enim salva matre fieri posse mihi persuadere nunquam potui, propter necessariam vulneris in epigastrii musculis, uterique substantia magnitudinem. gravida enim et parturienti mulieri cum uterus multo sanguine undique distentis vasis turgeat, ingentem inde haemorrhagiam, prorsumque lethalem consequi necessum foret Deinde consolidato uteri vulnere, haud permissura est obducta cicatrix ipsum denuo ad novae prolis gestationum dilatari. His aliisque ex causis id curationis genus, quod periculi et desperationis plenum est, minime usurpandum censeo." vi Opera chirurgica collect. per Petrum Uffenbachium. Francofurti 1590. p. 519.

[**] Elemens de la Science des Accouchemens, Pref. p. viii—xx. Lucine Française No. II.

The **Thalmud**, which owes its origin to the learning of the ancient Jews, and is a production of great antiquity, contains minute accounts respecting this operation, all of which evidently relates to its performance on the living subject. Even in the *Mischnajoth** which may be considered as the oldest work which the Jews possess, we find mention made of this operation under the name יוצא דופן (division of the parietes); and in the eighth division of the work is contained the following passage. "In a case of twins, neither the first child which shall be brought into the world by cutting into the abdomen, nor the second can receive the rights of primogeniture, either as regards the office of priest, or the succession to the property." Both the **Talmud** and the **Mischnajoth** possess several commentaries from different learned Jews; and the passage above quoted has received from each almost a different interpretation; but from all it is very apparent, that the operation was occasionally practised on the living subject. The indications for the performance of the Cæsarean operation are not contained in these works, and are only to be found in modern times; since it is by taking the different dimensions of the pelvis only, that it can be ascertained whether delivery can take place in the natural way or not. If we continue our researches farther back, we shall find other proofs of the Cæsarean operation being of older date than that generally ascribed to it by the learned. In a work called the NIDDA, which may be considered as an appendix to theTHALMUD, we find (Div. 5th) in allusion to the time during which the husbands were to abstain from their wives after delivery, " that it was not necessary for the woman to observe the days of purification after the removal of the child through the parietes of the abdomen." What then can be a stronger proof that the Cæsarean operation was known at that time than the above passage? and how natural is the conclusion that it was frequently performed on the living subject with success, particularly when one considers that, in the same work, (the NIDDA), there are several controversies respecting the point, whether it was necessary for women after delivery by this operation, to observe the days of purification. MAIMONIDES, who was written a commentary on the NIDDA, enters into an explanation about the words דופן יוצא which, according to him, signify, that a woman, who cannot bear in the natural way, should be opened in the side, and in this way delivered of her offspring. From this also we may see, that the incision in the *linea alba* is not the oldest mode of performing the operation, as OSIANDER * thought, but that the incision on one side claims the priority. How the operation was performed, SALOMO JARHI, a very learned Rabbi, who has written a commentary on the NIDDA, informs us, in the following words:—" The abdomen must be opened by SAMM, the child extracted, and then the parts healed." SAMM, in this passage, has a different signification from that in which it is generally employed: It usually means AROMA, or a substance which penetrates every thing by its smell; but here it evidently signifies an instrument, which was sufficiently sharp for the division of various parts.

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* The *Mischnajoth* was composed 140 years after the birth of Christ by all the learned Jews then living. Some even think it still older.

These are the results of Dr MANSFELD's investigations, and they form, in our opinion, " a valuable addition to the history of the Cæsarean operation. The point which Dr M. has endeavoured to prove was doubted by OSIANDER, who, only a year or two before his death, said, that he did not think the removal of the child, through an incision on either side of the abdomen, had been performed earlier than the operation in the *linea alba*, viz. the 16th century, but that it would be extremely remarkable if it could be ascertained that the operation on the side had been performed earlier either on the dead or living subject. Dr Mansfeld has proved, as far as the thing admits of proof, that the operation was performed, in early times, among the Jews, although no particular cases are mentioned, (for such are not to be found on record), still the operation is so frequently mentioned in the old rabbinical writings, that the fact of its having been performed long before the 16th century seems placed beyond all doubt.

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* Handbuch der Entbindungskunst, 2ter Bandes, 2ter Abth, p. 300, § 155.

We see then that with the ancients the Cæsarean operation was always performed on the side: the left side was first chosen in order to avoid the liver; afterwards it was performed on both sides according to the situation of the uterus, and the supposed situation of the placenta. The incision was made on the external side of the recti muscles, and parallel to them; frequently it was carried in an oblique direction towards the pubes. The bleeding which attended this mode of performing the operation must have been very considerable, from the frequent wounding of the epigastric artery; but still it is a curious fact, that from 1500 to 1769, a space in which, according to Stein, 82 Cæsarean operations were performed in this mode, only six turned out unfortunate. First between 1770 and 1780, some celebrated accoucheurs, both in Germany and France, began to adopt the linea alba as the best spot for the incision. This operation is ascribed by some to Z. Platner, but Mauriceau knew it, and Guerin first performed it.* In Germany, Henkel first (1769) attempted the operation in the linea alba, but Deleurge revived it, and named a surgeon of Lisle, one Varoquier, as its first proposer. Deleurge† was the chief person who showed the advantages which were to be derived from this mode of performing the operation, by preventing the protrusion of the intestines, and the loss of blood. Since that time, the linea alba is the spot which has been generally chosen. Shortly after this period Lauerjat recommended quite a contrary direction for the incision. According to him, the uterus should be divided in an horizontal direction; and he proposed that the first incision should be made between the recti muscles and the spine, just below the third false rib, at the point towards which the uterus projects. He thought this mode possessed the following advantages: 1st, as the lower two thirds of the uterus remain uninjured, a cavity might easily form in which the effused blood would be received, without passing into the cavity of the abdomen. 2dly, The transverse wound of the uterus heals in the completest manner, since the contraction of this part takes place principally from above downwards. 3dly, That this was also the case with the incision in the integuments he endeavoured to prove, by the direction which the fibres of the muscles oblique and transversalis took. LAUVERJAT performed the Cæsarean operation in this mode twice with success, but it met with a great deal of criticism from his contemporaries, and was not at all generally adopted. G. W. STEIN * whose principal object in the treatment of persons on whom the Cæsarian operation had been performed, was to prevent the separation of the lips of the wound of the uterus, objected to the modes which had been before tried, because, first the longitudinal incision (either in the linea alba, or external to it), was very likely to produce separation of the edges of the wound from the pressure exerted on the uterus from above, and the opposition from below or within, and in the transverse wound, because one edge might very easily lap over the other. According to STEIN the middle, when the two modes, viz. in the diagonal direction, could provide the proper position of the edges of the wound, because the lips of the diagonal wound, as soon as the uterus was compressed in the larger pelvis, would be mechanically kept in apposition. For those reasons Stein proposed that the incision should be commenced from the end of the last rib, and carried obliquely over the abdomen towards the ramus of the pubes of the opposite side, so that the middle of the incision should come in contact with the middle of the linea alba. The danger of the Cæsarean operation, however, does not arise from the separation of the edges of the wound of the uterus, for in the greatest number of the cases which have been examined after death, this did not exist, and in those cases in which it does, the wound is not likely to be closed in the way in which Stein supposed. It is true that STEIN found a complete separation of the edges of the wound in one case which he examined, but the usual appearance is to find the lower angle of the wound gaping, whilst the edges of the upper half of the uterus are in close contact. Thus for instance, KLEIN found on examination of a person who lived four days after the operation the stomach and bowels distended with air, the uterus of the size of a man's two fists lying at the entrance of the pelvis, so that the wound rested against the bladder. The inner edge of the lips of the wound were in contact,but not adhering to to each other; at the lower angle only were they asunder; the

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* Guerin, Histoire des deux Operations Cæsares. Paris, 1750.

† M. F. A. Deleurge, Observations sur l'Operation Cæsarienne à la ligne blanche, &c. Paris, 1779.

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‡ G. W. Stein's Geburtshülfligbe Abhandlungen I. Heft. Marburg, 1803.

external edges stood one inch apart from each other. At the base of the uterus the parietes were quite thick; the neck, however, was quite thin. * MENDEL examined the corpse of a person who died a hundred hours after the operation, and found the uterus six inches in length, four and a quarter broad, and above two inches thick. At the upper half the edges of the wound were quite close to each other, but not adhering; inferiorly they were separated to the extent of an inch. † This separation at the inferior angle is not always met with as may be seen by perusing the cases in SIEBOLD's *Journal fur Geburtshulfe* II. Bd. III. Heft, from SERVACS, and those from WEISSENBORN, ‡ where the edges of the wound were formed in the whole length were found close to each other although not adhering. These and similar observations show the fallacy of STEIN's opinion, which he too hastily drew from a few isolated cases. Another mode of performing the operation by making a transverse incision in the integuments between the rectus muscle and the spine, was most distinctly recommended by LAUVERJAT, and after him DEMKER, § advised that the incision should be carried through both recti muscles to the extent of 7 or 8 inches in length.

The most celebrated Continental accoucheurs are now agreed, that neither of the proposed modes in every case is applicable, arising from variety of circumstances, as the structure of the integuments of the abdomen, the situation, form, and projection of the uterus. If one only regarded the structure of the integuments, the linea alba between the umbilicus and pubes presents the greatest advantage. In no part are the abdominal parietes thinner than in this; and the bleeding which, in other parts, is very considerable, is here very trifling; and in no part can the adhesion of the integuments be more safely and quickly effected than in the linea alba. Advantageous as the incision in the linea alba in general is, Dr SCHENK thinks it is not always practicable; and the rule which is now laid down on the Continent is, to cut opposite to that part where the projection of the uterus is the greatest. The reasons for this are, that the integuments of the abdomen lay closer to the gravid uterus, whilst the omentum and the intestines are pressed either above or to one side, so that an assistant can very readily prevent them from protruding in the way of the operator. If, it is said, the surgeon, regardless of the shape and situation of the uterus, always made the incision in one direction, it must frequently happen that the intestines would prolapse, and retard the operation. The linea alba is the spot which is generally chosen for the performance of this operation in England ; and it appears to be the best on account of the less hemorrhage and greater facility of producing adhesion here than in other parts. With respect to the protrusion of the intestines, it may, we think, be entirely prevented when the operator has two good assistants, one on each side of the patient, provided with large sponges, so that a larger surface may be compressed, than with the bare hands. We have had an opportunity of observing this plan carried into effect, by which the intestines were completely repressed.

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* Loder's Journal fur's Chirurgie, etc, II. B. 4. Heft.

† Vid. Siebold's Lucina, VI. Bd. I. H.

‡ Observ. duæ de partu cæsareo. Erford, 1792.

§ Dissert. sistens rationem optimam adm. part. cæs. Duisb. 1771.

We shall now consider the spot most eligible for the incision in the uterus, which has been as much the subject of dispute as the part best fitted for the division of the integuments. MILLOT,* who advocated the lateral incision of the integuments in this operation, has also recommended that the uterus be divided as much as possible to the side, about three or four inches removed from the fundus, in order to prevent the after-adhesion of the intestines with this organ. The objection, however, to the incision being made in this spot, is the greater number of vessels here. The danger also of wounding the broad and lateral ligaments, together with the Fallopian tubes, is almost certain ; on which account the lateral incision of the uterus is seldom practised. In general, it is recommended to divide the uterus in a longitudinal direction from the fundus to the mouth. The advantages of this mode are, that let the child be in whatever position it may, it can with facility be removed. In the transverse incision of the uterus, the sides are wounded, and one or both of the round ligaments can be very easily injured. In the diagonal direction, also, the extraction of the child may be attended with difficulty, if it should happen to lie in the natural position, longways in the uterus. In this case, the incision remains far removed from the fundus of the uterus, the breech of the child will be confined in it as in a sack, and thus oppose its easy removal. The lower point of the incision will be at a little distance from the neck of the womb, so that the extraction of the head will be attended with difficulty, and perhaps the turning of the child be required. The longitudinal incision has none of these disadvantages, and affords an opportunity of removing the child, according to the circumstances of the case, at one time with the head foremost, at another with the breech. The danger of wounding the bladder in the longitudinal incision is certainly greater than in the other cases; but no one would think of performing this, or similar operations, without previously emptying both the bladder and rectum. The next step of the operation consists in removing the child and the placenta, which is not in general attended with great difficulty when the incision has been made along the upper surface of the womb. If strong contractions of the womb should come on during this stage of the operation, the operator will naturally desist from the attempt till they have ceased. The mode of removing the child must depend a great deal on the nature of each individual case; and it is therefore quite unnecessary to lay down any general rules respecting it. If, after the removal of the child, the placenta is in its natural position, and has not been injured by the division of the parietes of the uterus, its separation with the hand is easy, and requires no particular instructions as to its performance. It however very frequently happens, that it is wounded in cutting into the uterus; and this is by some considered as a very serious circumstance, on account of the increase of hemorrhage. But how often has the placenta been divided without any considerable hemorrhage following. Even when the whole placenta has been divided with the knife, the bleeding has not been very considerable; and we are not aware of any case being on record of death from hemorrhage in this operation. LAUVERJAT was of opinion that loss of blood in a considerable quantity was of use, and even thought it a desirable point to wound the placenta in the operation, that this might take place. The injury of the spot where the placenta is attached has some analogy to the case where the placenta is seated opposite to the mouth of the uterus in the delivery *per vaginam*, and in the same way requires the speedy termination of the labour. When the placenta has been wounded, the bleeding part can be compressed or not by an assistant, as the nature of the case may require, and the operator proceeds to lengthen the wound as quickly as possible; he can either cut through the placenta, as in the case where only a small portion of it lies opposite to the incision, and thus effect a clear opening for the removal of the child; or, where the whole placenta lies opposite to the wound in the uterus, continue the incision between it and the womb, and then separate it with the hand. WIGAND, and with him STEIN, have recommended the placenta not to be removed by the wound in the uterus, but by the vagina, for which purpose WIGAND directs that the navel-string should be brought by means of a small curved stick through the mouth of the uterus into the vagina, and thus separate it, or allow it to come away of itself. We see no advantage whatever to be gained from this complication of the operation; and the mention of it is only interesting in an historical point of view, to show the different stages through which operations, as well as most other things, pass before they arrive at a state of simplicity. As soon as the uterus is freed from its contents, and it begins to contract, the bleeding generally stops. We know only of one case where the contractions of this organ were not sufficient alone to stop the hemorrhage. This is recorded in Siebold's Journal of Midwifery,* where the operator was obliged to take up a vessel, and thus prevented the farther loss of blood, which he had not been able to do by any other means. The blood which remains in the uterus, or any portion of the membranes which may hinder the discharge *per vaginam*, must be removed with warm sponges, and the surgeon proceeds to the fourth stage of the operation, the union of the wounds.

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* Observ. sur l'Operation Cæsarienne avec la Description d'une nouvelle Methode de l'operer. Paris, an. vii. p. 407.

The union of the divided surfaces of the uterus is in general effected by nature with slight mechanical assistance; and the great object is to promote, as far as possible, the regular contractions of the womb, upon which alone the process of adhesion depends. With this view, slight mechanical irritation on the uterus will be found most serviceable; and after the removal of the placenta and the coagula of blood, slight friction of the uterus, or pressure, generally produces contraction of the uterus, if these had not previously taken place. Several other mechanical means have been recommended for the procuring adhesion of the wound of the uterus, particularly by those who were of opinion that the separation, as well as the adhesion of the lips of the wound, depended on mechanical causes. STEIN,‡ after advocating the diagonal incision of the uterus, under the impression that this was most favourable for procuring the adhesion, afterwards recommended the introduction of a sponge into the cavity of the uterus, either by the wound or the vagina, as might seem most practicable. Stein supposed that the sponge would afford a protection to the uterus against mechanical causes without, and serve for the taking up the secreted blood, and also the injections which were to be made use of after the operation. The sponge was to be provided with a string, which must hang out of the vagina, so that it might be withdrawn when thought necessary. This same accoucheur also recommended a graduated compress to be applied above the pubes, by which the lapping over of the lips of the wound were to be prevented. If any other mechanical means were necessary than those already mentioned, we should certainly be inclined to give the preference to the suture, which would not be likely to produce the same bad effects as Stein's mode. LAUVERJAT* mentions a case where the suture was employed. On the 27th of August 1769, LEBAS, surgeon at MOUILLERON, was called to a woman who had been four days in labour. Conceiving the Cæsarean operation indispensable, he made a transverse incision, which extended from a little below the navel to the ribs on one side. As this incision was too high, LEBAS made a second, more oblique than the first, which began about an inch under the navel, and joined the other. A complete protrusion of the intestines took place. A transverse incision was made in the uterus, which was brought together by a suture. The patient was neither bled nor purged. She received some decoction of bark as a beverage. As soon as the edges of the wound were threatened with gangrene, some of this decoction was applied to it. Purulent discharge followed from the wound, and the thread was withdrawn by means of which the wound of the uterus had been brought together. On the 8th of October the woman was completely cured, and on the 20th she was able to go about her usual occupation. We can conceive that one or two sutures to the uterus may not produce any bad effect, and succeed in effecting a speedy adhesion of the lips of the wound; but the results of the *post mortem* examinations of the fatal cases of the Cæsarean operation, show that union generally takes place to a considerable degree. We have stated, in another part of this article, the appearances of the uterus which are usually found soon after death from this operation; and cases are not wanting, from which it might be shown, that complete adhesion of the wound of the uterus sometimes follows very rapidly after the operation. In RICHTER'S BIBLIOTHEK,† is a case related by SCHUZER of a person dying four days after the Cæsarean operation, from an error in diet. The wound in the integuments of the abdomen were quite healed; the uterus had so firmly contracted, that it was scarcely so large as a fist, and the wound in it was nearly closed. In Dr SCHENK'S case it was found, on external examination, on the 10th day, that the womb adhered to the edges of the external wound. It is also probable that this had taken place still earlier, and would have been discovered if a deeper examination had been practicable. In

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* Fur Geburtshulfe, Vol. I. Part II.

† Geburtshuelfliche Abhundlungen.

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* Nouvelle Methode de Pratiquer l'Operation Cæsarienne, Paris, 1778.

† I. Bd. p. 90.

SCHMUCKER's *Vermischte Schriften* * is a case illustrating how well union, even under the most unfavourable circumstances, may take place. In the year 1779, the Cæsarean operation was performed by Dr FRITZE on a person well forward in the 6th month of pregnancy, whose abdomen had been ripped open by a mad bull, so that the arm of the child protruded. The woman recovered, again became pregnant, and was readily delivered in the natural way; she died, however, a few hours after delivery, from the rupture of some varicose veins in the ovary. On examining the uterus, it was found that the spot where the Cæsarean operation had been performed had so well cicatrized, that it was almost difficult to point it out with certainty; and the place where the bull's horns had penetrated would scarcely have been discovered, if the uterus had not been exceedingly thin here. These and similar cases show that the application of sutures to the wound of the uterus is not at all necessary.

The mode of keeping together the external wound has also been the subject of considerable dispute; some contending, that the position of the patient on the side, with adhesive plaster and bandages, are quite sufficient; others, that the use of sutures is indispensable. The first of these opinions, which is maintained by SABATIER and others, has been opposed by Siebold, who contends, that sutures are absolutely necessary, where the intestines are prolapsed, and cannot be returned in the usual way. In such a case, the integuments of the abdomen will require to be kept in apposition by some stronger means than simple adhesive plaster. This occurred in Dr SCHENK's case; so that, without the use of sutures, the intestines could not have been kept in their natural situation; and something similar occurred to the deceased MURSINNA (formerly surgeon to the Hospital (Charité) at Berlin), which compelled him to resort to the use of sutures, after trying for a long time in vain to keep the intestines within the cavity of the abdomen, by means of adhesive straps. MURSINNA says, † that "after the contraction of the uterus, the intestines protruded so forcibly, that various means were tried in vain to effect their apposition, and keep the wound together. The intestines were distended with flatus, rolled under our hands, and protruded out of the wound. I had never anticipated any thing of the kind, and never saw it before. It is indescribable with what pains they were obliged to be kept back. It appeared as if the cavity of the abdomen was too small to contain them, and I was obliged to relinquish my plan of producing union of the wound by means of adhesive plasters, and to employ sutures. The advantages of the firm union which was in this manner effected, were very apparent. The union which took place was so firm, that on the second day after the operation, on the occurrence of violent vomiting, some blood merely escaped through the lower wound, which, however, soon disappeared. The patient died fifty-eight hours after the operation, and the wound was so firmly united, that it could not be easily separated." Sutures are in general employed on the Continent, and without any bad effects being observed to arise from them. AUTENRIETH * proposed that the thread should be introduced into the integuments prior to the incision being made in the uterus, in order to prevent the too long exposure of the intestines to the atmosphere, and others have gone so far as to recommend the application of the adhesive plasters round the loins prior to the division of the integuments. The first of these propositions may be entitled to some attention, but the second must strike every one to be quite useless. The lateral position is in general advised, in order that the discharge may find a ready exit.

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* Bd. 3. 1788, page 64.

† Vid. Journal fur Chirurgie, B. II. 1 St. p. 254.

We cannot conclude these observations on the Cæsarean operation, without alluding to some alterations which have been proposed in the mode of performing it; which may be somewhat the more interesting, as they are not generally known in this country, and have emanated from men of considerable rank in their profession. OSIANDER, the late Professor of Midwifery at Gottingen, † recommended the following plan to be adopted in those cases where the head lay towards the mouth of the uterus. The operator must introduce one hand into the pelvis, lay hold of the head of the child, and press it towards the integuments of the abdomen. The external projection determines the spot for the incision. The surgeon then, with the other hand, divides the integuments to the extent of four inches, not longer, so that it might correspond with the lower half of the uterus, which must then be divided. The operator then pushes the head through the opening in the womb, and afterwards the rest of the child. OSIANDER thought that the wounds in this mode would be shorter than in the other way; and that after the escape of the head, the body would be pushed forwards by the contractions of the uterus, by which means he should avoid the danger of the head being retained by the contracting power of the womb, as is sometimes the case when

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* Tab. Blatter, 1816. II. 1. Heft.

† Osiander's Handbuch der Entbindungskunst, II. Th. II. abs.

the body is first removed. He expected, further, that the low situation of the wound on the contraction of the uterus would, in part, tend to prevent prolapsus of the intestines, and afford a freer exit to the discharge through the vagina, and above all, he thought that the incision in the lower half of the uterus less dangerous than in the upper part. The objections which have been brought against OSIANDER's plan are, that the introduction of the hand into the narrow pelvis and the undeveloped vagina, and the pressure of the head against the integuments of abdomen, may be attended with injurious consequences both to the mother and the child. It has also been doubted whether much would be gained in the length of the incision in this way. It has also been supposed that the wound of the neck of the uterus would be more likely to remain ununited than in the other parts of this organ, on account of its being a less muscular part. The opinion respecting the incision in the lower part of the uterus being less dangerous than in the superior portion, is not generally adopted, although Siebold and a few others have advocated it. SIEBOLD says,* "that the wounds of the uterus are most dangerous on the sides at the lower segment; and that those of the mouth, the base, anterior and posterior surfaces, are the least likely to produce any bad consequences."

2. The idea that the principal danger of the Cæsarean operation consisted in the wounding the uterus, induced Jörg † to endeavour to imitate nature, when she expels the child into the cavity of the abdomen by the rupture of the vagina, as is often the case, and for this purpose recommended the vagina alone, and in cases of necessity, the mouth of the womb to be divided, thus leaving the body of the uterus untouched, and through this opening extract the child. However impracticable this mode may appear, JÖRG was still surpassed by aother surgeon RITGEN, ‡ who, conceiving that the wound of the peritoneum was unnecessary, recommend the following operation: A semicircular incision is to be made in the integuments, extending from the crista of the os ilium on the right side to the pubes, and then carried in the same direction through the muscles; the uninjured peritoneum must then be separated with the fingers or the handle of the scalpel from its attachments, in order to allow the posterior portion of the vagina being reached, and in this way the incision terminated. All these

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* Lehrbuch der Entbindungskunst. I. Th. 1812, § 484.

† Handbuch der Geburtshulfe, Leipsic 1807, Div. x. cap. 7.

‡ Ritgen's Anzeigen der mechanischen Hulfen bei Entb. Giesen. 1820, p. 400.

proposed alterations appear to have no advantages sufficient to recommend them, and are therefore never adopted. In consequence of pregnancy sometimes occurring after the performance of the Cæsarean operation, it has been recommended by Michaelis to obviate this danger by the total extirpation of the gravid uterus,* which object it would most undoubtedly accomplish, by taking away the life of the patient.

With respect to Dr Schenk's case, the following are the chief particulars. The patient was a woman about 38 years of age, and already the mother of six children, her last delivery took place in the spring of 1819, when she brought forth twins. Since that period, however, she had laboured so much under disease, that the delivery in the natural way was impracticable. On the 2d of July 1823, the operation was performed by Dr SCHENK in the linea alba, and the child in this way readily removed. The integuments were brought together by sutures, and in about eight weeks after delivery the wound was completely cicatrized. About the beginning of October, exactly three months after the operation, the menses appeared. The discharge was stronger than usual, and what was very remarkable, the blood came not only through the mouth of the uterus, but also exuded through the middle and broadest part of the cicatrix, where the uterus had adhered to the integuments. Dr SCHENK had an opportunity of observing this appearance himself, which as far as he is acquainted with, has never been described. In the circumference of the cicatrix, the uterus could be distinctly felt in a state of turgescence from the menstrual congestion. In the menstruation which succeeded this, the exudation from the cicatrix also took place, but in a less quantity than before. Since this the menses have always appeared in the regular way.

The Cæsarean operation is now become so common on the Continent, particularly in Germany, that every journal for midwifery is filled with accounts of its performance; and the best sources of information to which we can refer for recent cases of this operation are Mende's Journal of Midwifery, † and Siebold of Berlin on the same subject. The performance of the operation has of late become much more frequent, and the success attending it (proportionally speaking) less than formerly. The circumstances under which the operation is recommended to be performed are, 1. when the conjugate diameter of the pelvis is less than 2 inches and a half, and the child is alive; and, 2. when the pelvis is so contracted that the removal of the child (supposing it to be dead) piecemeal is not practicable. Even these rules, we fear, have not been always strictly observed; and we know a case where every preparation for the Cæsarean operation was making, and its performance determined on, and where the woman, before the necessary things could be got ready, was delivered in the natural way. Unfortunately, also in Germany, it is deemed contrary to law to sacrifice the child, in order to preserve the mother; and the lives of the latter are on this account frequently exposed to danger, and sometimes destroyed, where they would, most probably, have been saved by the destruction of the child. The time which is, in general, chosen in Germany for the performance of the Cæsarean operation is when the neck of the uterus is expanded, the inner mouth dilated, and when the waters are not yet discharged. In England the operation is generally done much later; and to this its almost constant failure is attributed. It is difficult to say with accuracy the proportion of cases which succeed, because the operation is not unfrequently performed without any account of it being afterwards published. It has been said that the number of successful cases was formerly one to two, and now as one to three; but we believe the last of these numbers even to be incorrect, and that the number of those saved by the operation is very much smaller. There are certainly some very curious cases on record where the operation has been performed twice, five, six, and even seven times with success, some of which are to be found in the *Commentat. Societ. Reg. Scient.* (Vol. ii. Gott. 1813), in the *Russische Sammlung fur Naturwissenchaften und Heilkunde*, (B. i. Heft. 4.), and various other works.

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* Vid. Siebold's Lucina, V. B. § 89.

† Journal fur Geburtshulfe, Gottingen.

PART III.

MEDICAL INTELLIGENCE.

An Account of the Present State of Medicine in Italy, by Fr. W. Oppenheim, M.D.

The above is the title of an article in a late Number of the Magazine of Foreign Literature, an excellent medical work, edited in Hamburgh by Dr Gerson and Dr Julius. It contains much interesting matter, from which we have made a selection, calculated to give a correct idea of the state of medical science in Italy, and to serve as a guide to those who visit that country for the purpose of adding to their stock of professional information.

Our author describes the Institutions of the Italian States, according to the order in which he visited them, beginning with the kingdom of Sardinia, in which there are two Universities, one at Genoa and one at Turin.

In the Genoese University ten professors are employed in teaching the different branches of Medicine and Surgery. None of these professors however enjoy much celebrity; practical anatomy is shamefully neglected, and there is no anatomical museum. The languishing state of science in this University, is attributed by our author to its being under the direction of the Jesuits, who are in possession of its revenues, and expend considerable sums upon the purchase of theological books, while they almost entirely neglect, or give but little encouragement to the cultivation of Natural History, Comparative Anatomy, and the other departments of medical education. During the short-lived constitution of thirty days, the students ranked themselves on the side of the anti-royalists, in consequence of which, the University was closed for three months, upon the return and restoration of the king.

The population of Genoa amounts to 90,000. It has two civil, and one military hospital, besides a work-house.

The *Ospedale Pammatone* is very large, and externally resembles a palace more than an hospital. Its pillars, stairs and balustrades, all of Carrara marble, lead the stranger to expect a commodious interior. But here he is miserably disappointed, and finds a total want of every thing suited to promote the health or comfort of the patients. The wards are very spacious, but badly lighted, imperfectly ventilated, and extremely filthy. The floors are made of tiles, half worn out, and scarcely ever cleaned. The bedsteads are of iron, have no curtains, and are ranged in three rows, two of which are so close to each other that they touch.

The patients are attended by nuns, belonging to the order of *Nostra Donna del Rifugio*. Three physicians and four surgeons are attached to this institution, which has 1600 beds, but is capable of containing 4000. When our author visited it in 1824, the number of patients amounted only to 826. Post mortem examinations are here very rare, and the anatomical cabinet consequently very poor, containing only about a dozen dried preparations, and one skeleton! In 1821, the admissions amounted to 9344; the proportion of deaths to recoveries as 1 to 6.

Spedale degli Incurabili, is a handsome but badly situated building, containing 1000 beds, and destined for the reception of the aged, the poor, and the insane.

Patients of the latter description occupy a separate wing of the building. Their wards are spacious, but here again we meet with three rows of beds; and our feelings revolt at the situation of these wretched beings, the greater number of whom are chained hand and foot to their iron bedsteads! Where such a mode of coercion is employed,—where the strait waistcoat is unknown,—where the physicians, Drs Isola and Timoni, (we wish not to conceal their names), hurry daily through this abode of misery, without giving a single direction to the nurse-tenders concerning the treatment of the patients, can we wonder that a cure is scarcely ever effected? We are sorry to find, that disgusting as such a scene must be, it is even surpassed in an asylum at Vienna, where Dr Oppenheim has seen the lunatics not only chained, but caged, like beasts in a menagerie!

We feel particularly anxious that such a state of things should not exist unknown, and consequently unredressed. What Dr Oppenheim has already disclosed to our professional brethren in Germany, we think our duty to publish in Great Britain, in the hopes of thereby contributing to promote a reformation in the treatment of lunatics in Italy, the first step to a reformation being to attract public attention to the extent and nature of the abuse. While we feel it our duty to record this barbarous treatment of lunatics in one of the first cities in Italy, we are yet far from wishing to stigmatize the Italian character as inhumane, or their physicians in general as ignorant, for we ourselves recollect the existence, and that at no very distant period, of abuses not less enormous in Great Britain. *

Dr Oppenheim is doubtful whether to attribute to absolute want, or a degraded state of national character, the alms-begging which prevails in the Italian hospitals. " As I passed each bed, he observes, its sickly tenant stretched forth his meagre arms to implore for charity!

Albergo dei Poveri, a splendid building, adorned with costly architectural ornaments, but deficient in more essential qualities. It contains 2000 persons, consisting of the poor, the aged, and many orphans. The internal management of this institution is much better than that of those already described. The paupers and orphans appeared clean, well clothed, and well fed.

In Genoa there are also a military hospital containing 800 beds, and an institution for the instruction of the deaf and dumb. The latter was founded in 1801, by the Abbé Octavius Assarotti, who still presides over it, and pursues nearly the same mode of instruction as is usual in France. In Italy, as elsewhere, this malady has been observed to be much more frequent among the poor than among the rich. The state of this institution, which contains 20 boys and 12 girls, is highly creditable to the Abbé.

Turin, for a population of 80,000, has four civil hospitals. That of S. Giovanni is the largest, being capable of containing 600 patients. But at the time of Dr Oppenheim's visit, there were not more than 200 in it. The wards are large, and well ventilated. The proportion of deaths to recoveries, out of 4557 patients admitted in 1821, was as one to seven. There is a clinical ward in this hospital, containing 20 beds.

Casa dei Pazzi contained 260 lunatics, of whom more than one-third were chained. Their situation seemed even more deplorable than that of their fellow-sufferers at Genoa. Although there are three physicians and three surgeons attached to this asylum, yet Dr Oppenheim could not discover that any curative measures were ever employed.

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* For an acconnt of the dreadful and even appalling abuses in British Lunatic Asylums, see an Article on that subject in the Edinburgh Review, August 1817. See also the Report of a Committee appointed by the Irish House of Commons, to examine into the causes of the mortality observed among the children admitted into the Foundling Hospital in Dublin. Two-thirds of the children admitted perished from neglect or maltreatment. The cause of death in many cases was opium, in the form of a mixture, left in the hands of the nurses, who administered this *sedative* when the children became uneasy, or cried perhaps from hunger. Great numbers of *healthy* children were put at once into the condemned or venereal ward, in which, of course, no wet nurses were employed, and the infants were so negligently attended, that a majority of them perished in the course of a few days. This practice had existed for many years, during which it was *proved* to have been fatal to many thousands. The physician of the hospital, during that period of infantine slaughter, obtained the name of Herod the Great!

The University of Turin has eleven medical and surgical professors. The number of medical students generally amounts to nearly 100. The medical practitioners of the kingdom of Sardinia are much divided as to the systems they pursue. The juniors who have studied in Paris adhere in general to the doctrines of Broussais. Some of the seniors are still Brunonians; and what is singular, the younger Rasori has many fewer followers than might be expected.

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The Grand Dutchy of Tuscany has one University at Pisa, and three medical academies, viz. at Florenz, Pistoia, and Sienna. In order to obtain a license to practise, the student must not only have attended an hospital in one of the above towns for seven years, but must have practised during that period under the direction of the clinical professor, and must finally submit to an examination. Even those who have taken the regular academical degree of M. D. elsewhere, must attend one of the Tuscan hospitals for two years, before he is allowed to enter on private practice.

The academies at Pistoia and Sienna are too inconsiderable to claim attention. Our author therefore passes at once to the University of Pisa, which has nine Medical and Surgical Professors. There is here, however, no school for teaching midwifery.

The University building is small and inconvenient, and contains a few lecture rooms, besides an Anatomical and Surgical theatre, but the latter is so badly lighted, that most of the operations are performed in the former.

The medical courses commence in November, and conclude in August. The students of medicine amount to about 200, of whom many are Greeks. Dr Oppenheim had an opportunity of hearing one of the lectures on anatomy, and declares it was the worst he ever heard.

We shall give our author's account of the Medical Institutions at Pisa, in his own words.

" *Ospedale Santa Chiara é Casa dé Trovatelli* contains 300 patients, the wards are clean, spacious and lofty. The medical and surgical patients, in this as in the other Italian hospitals, are not separated from each other, but lie in the same ward. There is a clinical ward with twelve beds for the treatment of medical cases, under the direction of Professor Morelli, besides one for surgical cases, under the direction of Professor Vacca Berlinghieri. The attention of these professors is not however confined to the clinical patients, for they make remarks upon every interesting case in the hospital. The surgeon's visit is made at seven in the morning, the physician's at ten. The hospital pupils here, as in all the other Tuscan schools, wear a particular uniform, consisting of a red surtout, and a white apron; as the domestics of the hospital are similarly dressed, they are scarcely distinguishable from the Students. Professor Vacca Berlinghieri is extremely polite to strangers, and is very communicative upon professional subjects. I shall relate what I saw and heard while in his company. His method of lithotomy * is well known from his three memoirs on that subject. His success has undoubtedly been considerable; for of 29 patients upon whom he has operated, he has lost but two, and of these, one was more than 64 years old. I saw two persons upon whom he had operated; one was a boy 4 years old, who had been cut for the stone five days before. He seemed to be doing well, the greater part of his urine being already voided through the urethra, while the wound was beginning to heal. The other was a very melancholy case of a young man, on whom the operation had been performed 14 weeks previously to my visit. The stone had been broken in the first attempt to extract it, and the fragments were successively removed, great care being taken by the Professor to leave no part of it in the bladder. He used injections and every other usual precaution, but nevertheless, and although the wound had assumed an healthy appearance, the patient complained of calculous pains on the twelfth day, when the Professor discovered another piece of the stone in the bladder. In removing this he made no new incision, but merely dilated the original wound with his finger. This fresh irritation caused a violent inflammation of the parts, and the swollen part of the rectum, which had performed the office of a valve in preventing the feces from entering the bladder, sloughed away, so that a fistulous opening was formed between the rectum and the bladder, and the urine was consequently evacuated partly *per anum*, and partly through a catheter introduced into the urethra. When the catheter was introduced only as far as the neck of the bladder, the urine flowing through it was mixed with fecal matter, but when it was pushed higher, towards the fundus, the urine was natural. When I saw the patient, he was less hectic than he had been, but still the fistulous opening into the rectum remained; its calibre was however diminished. When this operation had been revived in France by Sanson, and improved in Italy by Vacca, who merely makes an incision into the neck of the bladder, it excited much attention, and was practised by many French and Italian surgeons with considerable success. Scarpa however urged many objections against its safety, the most important of which was the danger of wounding the vesiculæ seminales. To this Vacca replied by stating, that of 80 patients thus operated on, none had felt a diminution of their sexual powers. It is very remarkable that so celebrated a surgeon as Dupuytren should have so suddenly changed his opinion concerning the propriety of this operation. He now declares himself decidedly hostile to it, and yet when I was in Paris in 1823, he was quite enthusiastic in its favour, for I heard him say, that the number of patients who suffered from a fistula after this operation, was not greater than that of those who died after any other!

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* He opens into the bladder from the rectum.

"Calculous complaints are very rare in the neighbourhood of Pisa, and the majority of the cases operated on by Vacca, come from Bologna, Genoa, and the Piedmontese countries, whose population subsist almost entirely on vegetable nutriment; a fact proving that the formation of stone does not depend upon a superabundance of nitrogen.

"Diseases of the large arteries are so unfrequent at Pisa, that there had been no operation there for aneurism during ten years."

Dr Oppenheim however saw at Pisa one case of popliteal aneurism in a healthy man. It is singular that Vacca refused in this case to perform the operation; alleging as a reason, that he believed the patient's blood possessed too little *plasticity*,* and the arterial coats had too much inclination to suffer from distension or rupture, to authorize a reasonable hope of successful termination. He could not assign any intelligible grounds for this opinion, which was however justified by the event, for another surgeon having performed the operation, hemorrhage took place on the third day, which was stopped by tying the femoral artery above the origin of the profunda. This resource proved also ineffectual; for a fresh hæmorrhage occurred in three days after, and the patient died.

Dissection showed that all the coats of the artery had been divided by the ligatures, while no sufficient coagulum had been formed, and no attempt made towards the exudation of coagulable lymph.

What symptoms Vacca conceived to contraindicate the operation in this case, we as well as Dr Oppenheim are unable to guess.

"Fistula Lachrymalis is a very frequent disease at Pavia, and Vacca has observed it to occur in women more frequently than in men, in the proportion of 7 to 1. His operation consists in making an opening into the sac, with a small straight bistoury; he afterwards widens the sac with a fine silver probe, and introduces into the duct, a bit of extremely fine catgut, having a silk thread fastened to its upper extremity, by means of which it is secured above. In the course of a few days the lower end of the catgut is forced through the nostrils by blowing the nose; it is then drawn down and detached from the silk thread, to which he fixes a small dossil of charpie, and is thus enabled to introduce the latter, from below upwards, into the lachrymal sac. The advantage of this method is, that the dossil being introduced into the sac through the lower opening, the superior external opening need not be enlarged or stretched so as to render it liable to inflammation. Vacca insists upon the necessity of dividing the tendon of the orbicularis muscle in this operation, as a portion of the sac lies directly under this muscle, and of course cannot be touched with the necessary escharotics, and if this be not done, he contends that this portion of the sac will remain in a state of inflammation and will occasion a relapse of the disease. In my opinion the division of this tendon cannot be effected, without danger of also dividing the lachrymal duct.

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* The probable meaning of this expression is, that the blood was deficient in coagulable lymph.

" One of the most interesting cases I saw at Pisa, was an emphysema caused by fracture of the ribs on the right side of the thorax, together with injury of the pleura and lung, but without any external wound. The patient was brought to the hospital five days after the accident, when the emphysema was excessive, and had extended over the entire of neck, chest, abdomen and scrotum. The patient felt very little pain, and was quite free from the usual symptoms, cough and expectoration of blood. He could lie on either side, but preferred lying on the uninjured side. His respiration was free, and he could make a deep inspiration without its causing uneasiness. The transverse fracture of the rib was quite evident on examination. This case is certainly extremely interesting, and seems inexplicable. Under other circumstances it would have been necessary to make an immediate incision, but Vacca merely applied a few leeches, and the disease disappeared gradually.

" I saw here also a case of peculiar induration of the mammary glands, in a strong healthy woman aged 30, who had been but a few days in the hospital. Both breasts were equally affected, and were of a stony hardness; the indurated glands were moveable, and the skin immediately covering them was very red, while in every other part of the breast, it was quite natural as to colour. The temperature of the affected parts was elevated and the nipples as it were pressed inwards. The woman said that the disease had commenced suddenly and without any assignable cause, about seven years before, since which it had remained in the same state.

" In every other respect she felt herself remarkably well, did not feel any pain whatsoever in the breasts, and had no disorder of the digestive organs. There was no suspicion of syphilis, and the malady bore not the least resemblance to scirrhus; for *both* of the mammary glands were attacked at the same time, their temperature was increased, and the skin red. This disease too had remained nearly stationary from the time of its first origin ; and in its form the swelling did not at all resemble scirrhus, being destitute of the knotty elevations and inequalities so peculiar to that affection. The axillary glands were quite healthy.

" Vacca treated this disease as a case of chronic inflammation. He commenced with a venesection, the woman's constitution being very strong; and he afterwards applied leeches to the part, and cloths moistened with aqua laurocerasi. I saw this treatment continued for three days without effect.

" The operation of trepanning, which I never saw performed in France, and but once in England, during a very diligent attendance on the hospitals in both these countries, is by no means of rare occurrence at Pavia. Neither are its results on the whole unfavourable. In two cases a perforation was made through the mastoid process, for the purpose of giving exit to matter formed in its cells, in consequence of otitis. Vacca has lately abandoned an operation he was formerly in the habit of performing frequently, I mean tying the saphena vein in cases of varix and varicose ulcers. The success of this practice was for a time considerable, but cases afterwards occurred, in which dangerous symptoms were occasioned, such as violent inflammation of the vein. In a few instances this inflammation proved fatal. "

Dr Oppenheim observes, that an operation occasionally attended with such dangerous consequences ought never to be undertaken for the relief of a complaint in itself destitute of danger.

We subscribe most willingly to this opinion, having ourselves learned from an extensive experience the uncertain issue of tying the saphena vein. One case we shall not easily forget. A young person, otherwise enjoying perfect health, was admitted into an hospital in order to undergo this operation. It was performed. Inflammation of the venous system supervened, and the patient died in a few days!

For farther information on this subject, see Hodgson on Diseases of the Veins and Arteries, Mr Carmichael on Varix and Venous Inflammation, Dublin Hospital Reports, Vol. 2d, and an excellent article on "*Varices*" in the Dictionnaire des Sciences Medicales. *

Florence.—Population 80,000.

There are two hospitals, besides a foundling hospital.

1. Spedale de Santa Maria Nuova, capable of containing 1200 patients, but Dr Oppenheim found in it only 600 in April 1824. The wards are large and lofty. The lower wards are in the form of a cross, and contain from 150 to 200 beds. They are badly ventilated, and uncleanly. Six physicians and six surgeons are attached to this Institution, and attend in rotation, each for one month. Dr Oppenheim blames this practice, as subjecting the patients to a constant change of treatment. The clinical wards of this hospital contain 50 beds, and what is remarkable, almost all the operations are performed by the pupils, of course, however, in the presence of the professors. Dr Oppenheim saw here many cases of compound fracture, which were treated according to Dupuytren's method. He saw a case of medullary sarcoma affecting the testicles; castration was performed, but in five days a new fungous growth began to arise from the wound.

"Another melancholy picture was presented, by a case of fungus of the antrum highmorianum, which had forced its way into the mouth. Indeed I do not think I ever visited an hospital containing so many desperate diseases, such as hopeless cases of morbus coxarius, cancer, and abscesses, attended with confirmed hectic. Fistula lachrymalis is here also common; and I had an opportunity of seeing some cases of suppuration within the substance of the mastoid process, one of which proved fatal, from caries and effusion of matter in the dura mater. It was attended with symptoms of cerebral compression."

Scrofula in all its forms, such as tubercular phthisis, caries of the bones, &c. is not less frequent here than at Pisa. The Italians administer occasionally muriate of barytes in this affection, and Vacca often sends his scrofulous patients to the sea for the benefit of bathing.

The operation for cataract employed both at Florence and Pisa, is depression or else reclination, the needle being introduced through the sclerotica.

2. Ospedale di St Bonaficio, destined for the reception of lunatics, incurables, and those afflicted with cutaneous complaints, is capable of containing 1000 patients. The lunatics are here much better treated than at Pisa or Genoa; they are never chained, but are subjected to much milder modes of coertion, such as the strait-waistcoat; dark rooms having the walls lined with padding, to prevent the patients from injuring themselves, are used for the confinement of persons during the accession of the maniacal paroxysm. There is no care taken to provide amusements or employment for the patients, and on the whole, their *moral treatment* is entirely neglected, so that Dr O. justly remarks, it ought to be called an Asylum, not an Hospital for lunatics, the latter name implying the application of proper curative means; the consequence of this neglect is, that a cure is scarcely ever heard of.

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* *In varix* of the lower extremities we have never, when the disease has been of long standing, been able *to find valves in the diseased veins*. Our attention was drawn to this point by Mr Hewson of Dublin. When a portion of the saphena vein in such cases is excised, it immediately contracts so as scarcely to admit the passage of a probe. The inner coat of the vein is then found corrugated into longitudinal folds, while the external is considerably thickened, and possesses much elasticity and strength. This state of the vein appears necessary for resisting the increased pressure its coats have to bear when deprived of the mechanical assistance of the valves. This species of varix we would call *active varix*, to distinguish it from a much rarer species, in which great varicose distension of the veins takes place, without any increase of thickness in their coats. This we would call *passive varix*; it occurs in all parts of the surface of the body. We have been favoured by Dr Graves with a drawing of a woman at present in the Meath Hospital, Dublin, in whom the face, body, upper and lower extremities are disfigured with the latter species of varix. The disease commenced about her twentieth year.

Foundling Hospital.—A well managed Institution, which receives annually from 1500 to 1800 infants. They are well taken care of, and remain one year in the house, after which they are sent to the country. The bed-steads are of iron, and each contains four separate divisions, in which are placed four children's beds. This arrangement facilitates the attendance of the nurses upon the children.

" The diseases observed among the foundlings are not numerous. Inflammation or blenorrhoea of the eyes, is uncommon, a circumstance probably owing to the exclusion of a glaring light from the wards, and to their cleanliness and proper ventilation. Jaundice and induration of the cellular membrane, are quite unknown here. A great number of children *are said* to fall a sacrifice to syphilis: when brought into the house, they are apparently healthy, but in the course of a few weeks or even days, they become pale and thin, cry or rather *whimper* much, get an appearance of old age in the face, and often become covered about the genitals, anus, &c., with pustules and small ulcers. They grow cachectic, and finally die in a state of marasmus. The exhibition of mercury in this affection is found to be quite useless. I myself concur in the opinion of Dr Breschet of Paris, who has observed a similar complaint among the infants in ' L'Hospital des enfans trouvés,' but does not conceive it to depend upon syphilitic taint. His observations render it probable that it arises from an insidious inflammation of the abdominal viscera, a view of the subject confirmed by the diminution in its mortality, since a mode of practice founded on this view has been adopted.[*]

A small lying-in hospital, containing six beds, erected for the instruction of the Tuscan midwifes, is connected with the Foundling Hospital. In order to obtain a license to practise midwifery, the females must attend three courses of lectures on that subject, besides which, they must reside 18 months in the Institution. The anatomy of the female pelvis is taught, by means of beautiful wax models, which can be taken to pieces.

Puerperal fever is rare at Florence. On an average, twins occur 5 times in the 100. The Cæsarean operation has been performed twice there, and in both cases was unsuccessful. The midwifery practice seems on the whole judicious, and the accoucheurs are not addicted to the unnecessary use of instruments in delivery.

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[*] The species of itch, termed by German authors crusta serpiginosa, the true nature of which was first pointed out by Professor Autenrieth, is also frequently looked upon as syphilitic. *When of long standing*, the child becomes miserably emaciated, in consequence of the constant cutaneous irritation and want of sleep, while the whole surface of the body, together with the scalp and face, are covered with a papular eruption, the papulæ of which are inflamed, so that when they become very numerous, the skin assumes a red or copper colour, and extensive desquamations take place, together with superficial ulcerations about the anus, folds of the thigh, &c.

Here we shall only remark,

1*st*, That we have seen two cases of true pustular itch affecting the ears and face in adults. 2*dly*, That we believe the face in adults is so rarely affected with psora, only because constant exposure to the air tends to harden it, so that it is less liable to suffer from the contact of infectious matter. The hands are, it is true, equally exposed, but they only suffer in those parts which, by their situation, are less exposed, and consequently covered with a more tender cuticle, viz. between the fingers, the wrists, &c. 4*thly*, The face in infants does not enjoy this immunity from psora, because in them its cuticle is scarcely less tender than that on other parts. 5*thly*, When a *nurse and child*, both free from itch, are exposed to its infection, the disease *always appears first upon the child;* in other cases the nurse escapes at first, while the child being more susceptible of the infection, gets the disease, which it communicates to the nurse, in whom it will be then observed to appear first on the arm most *used in supporting the child.* These facts are always brought forward by the nurse as proofs that the disease is not the itch, but a spontaneous eruption occurring first on the child, and communicated to her. We have made these remarks in order to show the futility of the opinion which absolutely denies the psorous nature of any eruption which affects the face, and in order to lead to a more rational practice in this troublesome cutaneous affection of infants.

Professor Bigeschi speaks highly of the Ergot, as a means of forwarding the progress of labour in tedious cases.

"I must not omit mentioning the celebrated Florentine waxworks, which exceed the Vienna collection, not only in number, but in execution and anatomical accuracy. What has been added lately, is inferior to the old collection, especially in the colouring. I shall never forget my astonishment at seeing a representation of the distribution of the fifth pair of nerves. It left nothing to be wished for, and had every branch described by *a Bock* or *a Meckel*. In fact, on examining it, you could not determine which was the more to be admired,—the anatomist who made the dissection, or the artist who made the model. The late Professor Ucelli, who was not only an able anatomist, but an expert artist, enriched this collection with many beautiful specimens in comparative anatomy, well worthy of a minute examination. The imitations of plants, fruits, &c. are not less elegantly executed, but this part of the collection loses its value, from the circumstance that the objects represented are indigenous in Italy! and we do not find any imitations of rare or tropical plants.

"I did not observe so great a number of blind people in any Italian city as in Florence. Every good begging station in this city is occupied by a blind beggar, and those stations descend by hereditary right, from one generation to another. How great the profits of these beggars must be, appears from the answers of a young man, when asked, how it happened that he could afford to marry,—"*Thank God I have a blind father, therefore as long as he lives I can never want.*" In general, these blind beggars are attended by stout young men, so that the proper order of things is reversed, for he who can neither see nor work, supports him who can do both!

"Here I cannot omit adverting to another custom prevalent not only in Florence, but in the other Italian cities, and which must necessarily exercise an injurious influence on the state of the medical profession. The apothecary's shop is the physician's rendezvous, for his messages are left, not at his own house, but at the shop of the apothecary whom he patronizes, or who patronizes him.

"The Italians do not understand the comfort of the expression "at home," like us Germans, but spends all his leisure hours in the open air, in the street, and engaged in the "*Dolce far niente.*" The first thing the Italian practitioner does in the morning, is to hurry to his apothecary's shop, for the purpose of learning what orders have been left for him. Meetings are held by physicians, and appointments made at the shop of the apothecary, and there the young physician, who is looking for practice, *must* loiter away his days; I say *must*, for if he does not do so, he will not succeed. Every stranger who is in want of a physician, sends for one to the apothecary; and every one who has no family physician does the same. The *understanding* relative to their mutual interest, which arises from this singular connexion between these branches of the profession, must prove injurious to the patient, at least so far as it increases the probability of his being made to swallow medicine, more with the view of increasing the bill, than of restoring his health. This custom evidently degrades the physician, by making him a sort of creature of the apothecary, and likewise occasions a most serious loss of time, just at that period of life when his time is most valuable.

(To be continued.)

Observations on the Filaria or Thread Worm, found in the eyes of Horses in India. By W. Twining, Esq. Presented Oct. 2, 1824.—We have been indebted to Sir James M'Grigor for the perusal of some very interesting observations on the *filaria* found in the eyes of horses in India, by W. Twining, Esq. assistant surgeon to the forces, and surgeon to his Excellency the Commander of the forces in India, which was communicated, we believe, to the Medical Society at Calcutta, and will form a part of the first volume of these Transactions, about to be publisbed. The disease is by no means rare in India. Mr Twining also saw similar worms extracted from the eyes of horses in Ceylon, but they appeared to him of a darker colour and somewhat larger. Mr Gibb who had the charge of the stud at Ponah for 22 years, says, that it occurs exclusively in the cold months, never having seen a case before October, nor later than the commencement of March, and that the heavier the rains have been in their proper season, the more numerous have cases of worms in the eye been during the subsequent cold season It also occurred more frequently in the low situation and damp climate of Tirhoot than in Ghazepore, which is considerably higher, and in the upper provinces they are still less known. The worm is always found in the anterior chamber of the eye, swimming in the aqueous humour, and the usual result has been complete opacity of the transparent cornea, and subsequent loss of sight of the eye. Two worms are sometimes seen in the same eye at one time, and a second worm has made its appearance after the first has been removed by operation. The worm is easily removed, by puncturing the cornea in a proper situation by a common bleeding lancet, and making the aqueous humour spirt out, which carries the worm along with it. In a case detailed by Mr Twining, " After extraction, the worm resembled a piece of fine white sewing thread, seven-eighths of an inch long. Viewed with a microscope, this worm had the appearance of a large semitransparent piece of catgut. There were five luminous spots, disposed circularly, near one end, which I imagine to be the head of the animal; below these spots there was an irregular luminous patch, in size nearly the diameter of the worm; and from this, two parallel lines of a similar appearance extended the whole length of its body. On the day this worm was extracted and examined, the wind was remarkably dry, and consequently in a few minutes the worm became like a shred of isinglass, and I could not discover any aperture in its semitransparent substance. The end which I conceive to have been the head, and where the group of luminous spots was situated, was round, and very little larger than the other extremity, which was flat. " It is worthy of notice, that horses with worms in the eye are often affected with debility of the loins; and as worms precisely similar to those in the eyes exist also in the cellular substance of that animal, and are met with more particularly by the sides of the lumbar vertebræ in great numbers—a fact which was first observed by Dr Hardtman—Mr Twining's conjecture, that there is a connexion between the two affections, seems probable. But Mr Gibb and Mr Moorcroft, who carefully dissected horses weak in the loins, never observed worms in the cellular membrane of any part, or in the canal of the spine, but invariably found them in the stomach and intestines, in very great numbers, particularly in the cold months.

Fifeshire Medico-Chirurgical Society.—The Professional gentlemen of the county of Fife have united themselves into a Medico-Chirurgical Society, which is to hold meetings at stated times for mutual improvement, and the communication of professional knowledge. They held their first meeting at Cupar on the 1st November, which was most respectably attended by gentlemen from every part of the county. Dr Grace of Cupar was elected President; and from the unanimity and zeal which prevailed, there can be no doubt entertained that the Institution will prove highly useful and creditable to its members, and eventually contribute to the advancement of professional knowledge in the country.

General Assembly of German Physicians and Natural Historians.—Our German brethren have also their societies; but, as in every thing else, their natural enthusiasm carries them beyond the practice of other people. Not content with bringing together the members of our profession in districts or even kingdoms, the genius of Oken, if we mistake not, conceived the idea of assembling every year, in the principal seats of learning alternately, the physicians and philosophers of the whole German empire. The meeting of 1825 took place at Frankfort on the Mayne, and lasted during the 18th, 19th, 20th, 21st, and 22d of Septembr. It was numerously attended, and even foreigners from Russia and London were present. The reading of papers on a great variety of subjects connected with medicine or natural history occupied the mornings from ten to two. They dined together every day except the 19th, when they were entertained by the Senkenberg Society of Natural History, in whose apartments the meetings were held, and in the evening they formed smaller social parties. We may conclude in the words of Dr Froriep, from whom this notice is taken,—" Assuredly it is no small enjoyment to spend some days undisturbed, with so many individuals distinguished for their genius and knowledge, or esteemed for their experience, and to renew old friendships or to contract new ones. "

Extirpation of the Carcinomatous Uterus through the Linea Alba.—Professor

LANGENBECK * of Gottingen has at last undertaken the extirpation of a carcinomatous uterus through the linea alba; the patient, however, unfortunately died soon after the operation. Professor LANGENBECK will no doubt shortly publish an account of the operation, and establish a comparison between the advantages of this operation and that through the vagina. Professor SIEBOLD remarks, that the extirpation of the uterus through the linea alba was first recommended in the year 1814, by one of his pupils, Dr GUTBERLET, at present Stadt-physicus in SCHWEINFURT (*Vid. First Volume and Second Number of Siebold's Journal*, p. 228.) and that he has also given a description of an instrument for conducting the knife of the operator when he divides the uterus from above downwards. SIEBOLD, *Journal für Geburtshulfe, November* 1825 (*dated* 1826) *vol. 5th.*

Remarkable Case of Rupture of the Uterus, and of the Safe Delivery of the Woman by the Cæsarean Operation. By Dr Ludwig Frank, at Parma. Salzburger Medic. Zeiting, Feb. 1825. p. 255.—ANGELA GROSSI, æt. 44, native of PARMA, and the mother of five children, was taken in labour of her sixth child at the beginning of the ninth month of pregnancy, August the 9th, 1817. A midwife was called, who afforded her the necessary help; but as the patient was standing up, she was suddenly seized with vomiting and faintness, and was therefore immediately conveyed to bed by the midwife and attendants. At the instant she was laid on the bed, she felt something give way in the abdomen, and then, to use her own expression, it appeared to her as if there were two children in the womb. Under these circumstances, a surgeon was sent for, who recommended to her rest, as he conceived the sensations of the woman arose from the motions of the fœtus during the act of vomiting. But the midwife, finding that the abdomen was more and more distended, that the vomiting continued, and the breathing was difficult and interrupted, sent for Dr Joseph ROSSI, professor of midwifery. Professor ROSSI, on a minute examination, decided that the uterus was ruptured; and after consulting with his father Dr Francis ROSSI, and other practitioners in the town, he, in common with his colleagues, decided that the Cæsarean operation was absolutely indicated in the present case. The operation was performed two hours after the rupture of the uterus is supposed to have taken place, by Professor CECCONI, in the presence of the two Drs Rossi, Professor Pizetti and others. The incision was made on the left side of the abdomen, just in the spot where the feet of the child could be felt. After the incision was made, the feet immediately presented themselves to view; and the living child, together with the placenta, were then removed. Forty days after the operation, the patient was perfectly restored, and able to walk out. Her menses some time after this appeared; and in the space of three years from this period, the same woman was delivered of a seven months child, which lived fourteen days. Over the spot where the incision was made in the abdomen, a cicatrix of the size of an apple remained, which, although it could never be completely healed, caused the patient very little inconvenience.

This case Dr Frank considers in some respects very remarkable, and he has therefore determined on sending the details to the Medico-Chirurgical Society at MARSEILLES. He is at present only acquainted with two similar difficult cases, both of which occurred to the celebrated surgeon M. LAMNBRON in Orleans. In the one case he performed the operation eighteen hours, and in the second two hours after the rupture of the uterus.—*Vid.* art. *Rupture de l' Uterus* in the *Dictionnaire des Sciences Medicales, vol.* 49. p. 249.

Preventive Power of the Belladonna against Scarlet Fever, Hufeland's Journal der practischen Heilkunde, Nov. 1825.—Hufeland says, it gives him great joy to be able to confirm, by the results of fresh experience, the efficacy of Belladonna as a preventive against scarlet fever. Five years have now elapsed since in his Journal the request was for the first time made, to employ the belladonna with this view; and, since that time, every year has brought fresh proofs of its power. This recommendation, which originated in German ground, is beginning to excite attention in France and England, and we may at last hope that its real merits will soon be appreciated. Dr H. has used the medicine repeatedly in his own practice, and has never seen it fail in a case where it had been properly tried.

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* Since our last notice of the extirpation of the uterus, it has been performed in Germany four times, twice by Professor LANGENBECK, in one case through the linea alba, and in the other through the vagina; once by Professor SIEBOLD of Berlin, and also by Dr HOLSCHER of HANOVER. The two last operations were done by the vagina. None of the cases succeeded.—*Ed. of Ed. Journal.*

Observations on this subject by Dr Kunzman, Berlin—Since Dr **Brandt** called the attention of the profession to the preventive power of belladonna against the scarlet fever in the *51st vol. of Hufeland's Journal*, Dr K. employed it in every family in which he had an opportunity; in fact in several cases, and with one exception only, always with success. In this case, as he afterwards found, the medicine had not been properly used. But as we see the scarlet fever only attacking certain members of the family, and sometimes leaving whole families unattacked, some doubts still remained in his mind about the efficacy of this medicine as a preventive of the scarlet fever. In January, however, of this year, a case occurred in his practice, which tells very strongly for the alleged power of the belladonna. In an Institution of which he had the medical direction, were about 70 children, of both sexes, from 4 to 14 years of age. On the 25th of December last year, the son of the Director of the Institution was attacked with the scarlet fever, and before Dr K. was informed, by the 28th two girls, of 4 and 7 years of age, were also attacked. He now separated the sick from the healthy as much as possible; but a strict separation, on account of the circumstances of the Institution, was not practicable, and the propagation of the complaint among the rest of the children was very much to be apprehended. Dr K., therefore, ordered the usual mixture, composed of two grains of the extract to an ounce of peppermint water, of which, twice a day, as many drops were given as the children had years; twelve drops, however, being the largest dose which was given. This was strictly attended to by the Director. From the time this precaution was adopted, the disease made its appearance in no individual case. Only on the 23d of January, four weeks after the first patients were attacked, the scarlet fever attacked a boy of ten years of age, which, however, was of a very mild nature. The second son of the Director of the Institution, who took no belladonna, was attacked with the scarlet fever in a very high degree.

Dr K. directs the belladonna to be continued as long as any traces of the epidemic show themselves. In the cases mentioned above, the children took the medicine six weeks in succession, and he never saw any bad effect from its continuance.

On the infectious Power of Scarlatina.—Protection against infection can take place only in two ways; first, by the removal of the infectious matter, or by taking away the susceptibility of the system for it. To the latter class belong vaccination and the belladonna. But there still exist among many individuals imperfect ideas respecting the infectious power of the scarlet fever, which certainly is of importance as it relates to the consideration of the properties of preservatives against it. Nay more, there are some physicians who doubt the infective power of scarlet fever in the limited sense of the word, namely, by contact, and who hold the disease to be purely epidemic, depending on the state of the atmosphere. But it is not possible to conceive an epidemic disease arising from this cause, without admitting an atmospheric contagion (*Luft-contagium*), which H. thinks he has clearly proved, in a paper on this subject. † The distinction between the epidemic and contagious disease consists only in this, that, in the former, the contagion (considered as seed), in its first connexion with the body, and its first act of propagation, perishes, and loses the power of reproduction; in the latter case, on the contrary, it retains its reproductive power, and is capable of spreading the disease far and wide, from the epidemically attacked individuals. This last property can either be peculiar to the epidemic contagion, or it can be given it by accidental circumstances; for instance, heat, confinement of several patients in one room, &c. Thus arise the epidemic contagious diseases, *i. e.* such as are only in their nature epidemic, arising from the state of the atmosphere, and not usually propagated from individual to individual, but which, by a very high degree of their infective power, or a very great susceptibility of a subject, may in individual cases become contagious; and to this class of the *epidemic contagious diseases* belongs *scarlet fever*, since there are incontrovertible instances on record of its individual contagious power.

Non-mercurial Treatment of Syphilis.—In a late number of *Gräfe's and Walther's*

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† Uber atmosphärische Krankheiten und atmosphärische Ansteckung in LVII. Vol. of Hufeland's Journal, 1823, § 1.

Journal,[*] we find an article by *Dr Otto* of Copenhagen on Syphilis, particularly the treatment of it without mercury, and the prevalence of this practice in England, in which Dr O. has drawn the following conclusions.

1. That the cure of syphilis, without mercury, has been asserted by so many authorities, that the fact can no longer be doubted. If then the disease could formerly be cured without mercury, it may certainly now be much easier, as it has lost much of its violence and obstinacy.

2. Syphilis can undoubtedly be radically cured in this manner; but then the cure is of longer duration, and the diet requires considerable restriction.

3. The secondary symptoms, and a return of the complaint are certainly more frequent; but the symptoms are not so difficult of removal, and the treatment has a much more speedy effect.

4. As the treatment without mercury requires a longer time, it appears more practicable in hospital than in private practice, and, on the other hand, the patient can be better watched in a hospital, which, on account of the diet, is of great importance.

5. " As ulcers on the genitals are often not syphilitic, and the use of mercury is contraindicated from a predisposition to scrofula or phthisis existing in the individual, it is consolatory to learn, from the results of experience, that this medicine is not always necessary, and that a radical cure, by more simple and innocent means, can sometimes be effected. Where, however, the physician is anxious to avoid the possible evils which mercury is capable of producing, and also avoid loss of time, there remains a middle way, namely, to employ mercury—whose specific action can scarcely be denied—in moderate doses. "

It is gratifying to see the prejudices respecting the use of mercury gradually diminishing, especially in the north of Europe, where they are the strongest. In the south of Germany the change which has, of late taken place in the minds of several surgeons there, is so considerable, that the non-mercurial plan of treating syphilis has in several places been carried into execution. We cannot omit noticing here a work which has lately appeared at Stuttgart and TUBINGEN, by Dr HUBER,[†] in which the non-mercurial plan is advocated.

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[*] B. vii. H. 2.

[†] Bemerkungen über die Geschichte und Behandlung der venerischen Krankheiten von Dr V. A Huber, 1825, in 8vo.

APOTHECARIES' ACT.

IT is of importance to the Medical Public to be informed, that the Act for amending the Apothecaries' Act of 1815, which was granted last Session of Parliament for one year only, expires in August next; and at the same time to be aware, that an inquiry into the evils arising from the Apothecaries' Act is likely to take place at the commencement of next Session of Parliament.

It is of the utmost consequence therefore that the Universities, Colleges of Surgeons, and Faculties of Medicine and Surgery, in behalf of their Graduates and Licentiates, as well as the Graduates and Licentiates themselves, who, by the provisions of the Apothecaries' Act are restrained from the exercise of their profession in England and Wales, should be prepared to apply to Parliament for redress.

It is of equal importance to the inhabitants of England, especially of the Counties, who are, by this impolitic Act, prevented from employing as the ordinary medical attendants upon themselves and their families, the best educated classes of the Profession.

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Communications have been received from Drs Alexander, W. F. Bow, Briggs, and T. J. Graham; and from Messrs Coulson, George Forbes, C. W. M. T. Graham, Mitchell, and Roger Wakefield Scott.

The following Publications have been received—

Myology, illustrated by Plates, in Four Parts.

Part I. Muscles of the Anterior and Posterior part of the Thigh, Leg, and Foot; Part II. Muscles of the Anterior and Posterior part of the Arm and Hand; Part III. Muscles of the Abdomen and Back; Part IV. Muscles of the Face and Eye; Anterior and Posterior part of the Neck, with the Muscles of the Perineum, completing the whole of the Muscles of the Human Body. By Edward William Tuson, House Surgeon to the Middlesex Hospital. Imperial Folio. Part I. is Published, consisting of two Dissected Plates, with Explanations. London, 1825.

System of Anatomical Plates, with descriptive Letter-Press. By John Lizars, F. R. S. E., Fellow of the Royal College of Surgeons, and Lecturer on Anatomy and Physiology, Edinburgh. Plates coloured after Nature, Folio, and Descriptions. 8vo.

Part VII. The Brain; First Portion. Seven Plates, and 18 pp. 1825.

Part VIII. The Brain; Second and concluding Portion. Eight Plates, 47 pp. 1825.

Part IX. The Organs of Sense. Ten Plates, and 209 Pages. 1826.

A. Cornelii Celsi Medicinæ Libri octo, ex recensione Leonardi Targæ; quibus accedunt tituli marginales perpetui, capitum, librorumque; annotationes criticæ, medicæ, physicæ; tabulæ characterum, ponderum, mensurarum, aliæ; indices materiæ medicæ Celsianiæ, rerumque omnium locupletissimi; praefixa de Celsi vita dissertatione. Concinnavit Edvardus Milligan, M. D., S. A. SS., Collegii Regii Medicorum Edinburgensis Sodalis; Medicinæ Theoreticæ Praelector; Societ. Philos. et Liter. Mancuniensis et Societ. Philos. et Liter. Leodensis Socius. 8vo. Edinburgi. pp. lxviii et 486. 1826. (*Opus numeris omnibus absolutum*. Diurnorum Medico-Chirurgicorum Edinensium Editor.)

An Elementary Compendium of Physiology, for the Use of Students. By. F. Magendie, M. D., &c. &c. Translated from the French, with copious Notes and Illustrations. By E. Milligan, M. D., Fellow of the Royal College of Physicians, and of the Scottish Society of Antiquaries, corresponding Member of the Literary and Philosophical Societies of Manchester and Leeds, Extraordinary Member of the Royal Medical Society, and Lecturer on Physiology and Therapeutics, Edinburgh. Second Edition, greatly enlarged. pp. 618, 8vo. Edinburgh, 1826.

A Letter to Sir Astley Cooper, Bart., F. R. S., Surgeon to the King, &c. &c. on certain Proceedings connected with the establishment of an Anatomical and Surgical School at Guy's Hospital. By J. H. Green, F. R. S., Professor of Anatomy to the Royal Academy; Professor of Anatomy and Surgery to the Royal College of Surgeons, Surgeon to St Thomas's Hospital, and Lecturer on Anatomy and Surgery at that Hospital. pp. 75, 8vo. London, 1825.

An Essay on the Application of the Lunar Caustic, in the Cure of certain Wounds and Ulcers. By John Higginbottom, Nottingham, Member of the Royal College of Surgeons of London. pp. 147. 8vo. London, 1826.

Contributions towards the Medical History of the Waters and Medical Topography of Cheltenham, containing Directions for drinking the Waters. By John Fosbroke, resident Surgeon at Cheltenham. pp. 180, 12mo. Cheltenham, 1826. (This Work is published in a detached form, as well as together with a general history of Cheltenham.)

The Medico-Chirurgical Review (Quarterly). Conducted by Associated Physicians and Surgeons, and superintended by James Johnson, M. D., Member of the Royal College of Physicians, London, and Physician Extraordinary to his Royal Highness the Duke of Clarence, &c. Vol. VIII., Analytical Series; New Series 7th. January 1826. pp. 304. Price six shillings.

The Edinburgh Journal of Medical Science, exhibiting a compendious View of the Progressive Improvements and Discoveries occuring in Practical Medicine, Practical Surgery, Midwifery, Anatomy (Human, Comparative, Morbid), Physiology (Human, Comparative), Chemistry, Pharmacy, Botany, Materia Medica, and Medical Jurisprudence. Each of the Divisions enumerated will be conducted by Gentlemen devoted to the teaching of, or engaged in the actual Practice of these different pursuits. No. I. pp. 248. 8vo. January 1826. Price six shillings. To be continued quarterly.

Magazin der ausländischen Literatur der gesammten Heilkunde und Arbeiten des aertzlichen Vereins zu Hamburg, herausgegeben von Dr G. H. Gerson und Dr Nikol. Henr. Julius. November–December 1825. pp. 361–488, completing the tenth Volume of this well conducted Journal.

Observations on the Lepra Arabum, or Elephantiasis of the Greeks, as it appears in India. By Whitelaw Ainslie, M. D., F. R. S. (From the Transactions of the Royal Asiatic Society of Great Britain and Ireland. Vol. I. 4to. London, 1825.)

A Treatise on the Diseases of the Eye, including the Doctrines and Practice of the most Eminent Modern Surgeons, and particularly those of Professor Beer. By George Frick, M.D. Ophthalmic Surgeon to the Baltimore General Dispensary. A new edition, with Notes, by Richard Welbank, Member of the Royal College of Surgeons, and of the Medical and Chirurgical Society of London. With an Engraving. pp. 308. 8vo. London, 1826.

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PART I.

ORIGINAL COMMUNICATIONS.

I.

Case of an Eleven Months Pregnancy, with Observations. By JAMES COLLINS, M. D. Liverpool.

IN the beginning of October 1824, the husband of a Mrs D., residing in Shand-street, Liverpool, told me his wife expected to lie-in, in the close of that month, or the commencement of the next; and that she desired that I would, on that occasion, be accoucheur. Some days after, I called to make the usual inquiries, and to ascertain with more precision the stage and character of the pregnancy. She said, she was in its eighth month, and that she dated its commencement from the close of the month subsequent to the last appearance of the menses. It is unnecessary here to observe, that she thereby avoided the common error into which so many women fall in similar circumstances, by supposing themselves in the family way, from the time at which the menstrual discharge ceased to return.

The subject of this memoir is a woman about twenty-four years of age, stout, well made, a little above the ordinary height, and the mother of several children. At this time the fundus of the uterus was as high as the epigastric region, and gave to the abdomen the form and appearance it assumes at the end of the eighth month of pregnancy. Knowing how fallacious the opinions are that rest exclusively on the evidence of external symptoms, or on the statement of the patient, and being anxious to cultivate the knowledge which touching gives of the stages of pregnancy, I affected to perceive some urgent necessity of examining *per vaginam*, and succeeded thereby in procuring her assent to the proposal. Her size and appearance at this time were particularly similar to what authors describe as indicating this stage, and her respiration was frequent, in consequence of the pressure on the diaphragm and intestines, occasioned by the ascent of the uterus.

In examining *per vaginam*, I found the neck or cervix of the uterus remarkably high, scarcely tangible, and with difficulty distinguished from the body of the uterus, as it presented little or no prolongation. Availing myself of my position, I placed the left hand on the abdomen, and, giving a gentle jerk to the *os tincæ* with the index finger of the other, the fœtus bounded from the touch, and fell again on the finger, exciting the sensation which the French call *ballotement*, and that degree of weight which a fœtus of eight months, it is supposed, could alone produce. Thus I ascertained the stage of the pregnancy; as the indications I have just detailed, according to the experience and observation of the most eminent in the profession, are sufficiently characteristic of it, and constitute the most conclusive means we possess to determine it with accuracy. Hence I was enabled to appreciate the value that ought to be attached to the history of the symptoms which the mother gave of her pregnancy. It was satisfactory, and in some degree corroborated the result of the examinations and observations to which I induced her to submit. She told me she experienced all the ordinary symptoms of this condition; suppression of the menses, augmentation of the size and sensibility of the heart, the encircling areola, depraved appetite, and all the other concomitant feelings and signs of its various stages, in the same order and intensity in which they succeeded one another in her previous pregnancies. It is true, there was one exception in the phenomena of this case. The period of quickening was later. She usually felt it about the end of the fifth month, but the sixth was considerably advanced in this instance before she was sensible of the movements of the child; and, as if to atone for their delay, they were much stronger, more frequent and troublesome, than she had ever experienced in any pregnancy. But we cannot deduce any positive inference respecting the age of the fœtus from the presence or absence of this feeling; and in this view, we ought not to attach so much importance to this indication as people generally do, as we have authentic records of cases in which, during the whole period of pregnancy, mothers never experienced this sensation.

As the subject of this paper has been the mother of several children, and as she possesses a good understanding, an admirable moral and religious character, and had no motive to deceive, if it were in her power, as will appear from the subsequent history of this case, she must necessarily be supposed competent to judge and compare her previous feelings with the corresponding ones of this pregnancy; and, therefore, if in these matters there be any credit given to the communications of women, and if there be any truth in the medical means that enable us to ascertain the age of the child without reference or regard to them, this woman must have been, when first I saw her, eight months pregnant at least: and this inference becomes the more just and impregnable, from the consideration that her own account of the symptoms which, in the opinion of many eminent practitioners, indicate the stages of pregnancy, coincided with the result of the physical proofs, which, of themselves, enable us to decide these matters.

As the end of the ninth month approached, the uterus, in consequence of the ordinary laws of pregnancy, gradually sunk in the abdomen, and at last, seemed as depressed as I ever saw it previous to labour. She now began to feel all the sensations that usually accompany this stage, considerable tenesmus, frequent inclination to pass urine, pains and torpor of the legs, caused by the pressure on the bladder, rectum, and nerves of the sacrum, occasioned by the descent of the uterus. I was again induced to repeat the examination *per vaginam*, to which the study of midwifery on the Continent perhaps made me attach too much importance in determining the stages of pregnancy. I found the head of the fœtus much lower of course than before, seeming larger, and more solid to the touch. The *os tincæ* was in some degree open, flat, and stretched; in short every thing appeared to announce at the usual period the commencement of labour, and, in fact, she already began to feel all its precursory symptoms, resembling in every respect those she experienced at the same stage of her former pregnancies. A day or two after the expiration of the ninth month, these sensations became so urgent, that she was repeatedly obliged to dress *en déshabille*, in expectation that the approaching event was near. Alas, how deceived! Harassed and annoyed by the inefficacy of the pains, she sometimes placed herself in bed in the hope that true ones would soon free her from the anxiety of her situation. At other times she rose in disappointment, walked about the room, on the supposition that exercise and an erect position would accelerate their arrival. I suggested the propriety of this experiment, because I naturally supposed that the neck of the uterus would more easily dilate in consequence of the increased pressure which the head of the fœtus must necessarily produce on it in this position, and consequently, that effective pains were more likely to come on.

In this manner nine or ten days elapsed without any other effect than the excitement of sharp and transient pains, arising occasionally in the back, and then directing their course towards the pubes and thighs, varying now and then in force and direction. Every night during this time she felt unusually heavy, with a return of the same symptoms between eleven and twelve o'clock that are generally considered the precursors of labour. In fact they were so similar to them, that every day increased our confidence that she would lie-in before the arrival of the next; and this hope was particularly strengthened by the appearance of a considerable quantity of glairy mucus that now began to flow from the parts of generation. At first I did not attach much importance to the case, because I considered it one of those anomalous ones in which nature now and then protracts gestation a week or fortnight beyond the ordinary time. I therefore anticipated only a tedious and difficult labour. But when I saw three or four weeks pass in this way without its arrival, and when I saw the precursory indications I have mentioned gradually diminish, and vary in the regularity of their return, I became uneasy, doubted the accuracy of my own observations, and of her memory and judgment in the detail which she had given me of the symptoms of her pregnancy. I therefore resolved to repeat my observations, and to resume the examination *per vaginam*, not from the motive that suggested those I had formerly made, but from the desire of ascertaining whether I was mistaken in supposing her more than nine months pregnant, and of knowing whether there was any unnatural or unforeseen impediment to the expulsion of the child; but I found the parts just as they were at the close of the ninth month. It is true, the fundus of the uterus seemed to have fallen much lower than it was before, so much so that strangers observed it, and, in common with the mother, frequently said they never saw a woman carry her child so low. I became embarrassed. The signs and symptoms which practical writers detail made me believe I was right; but the extraordinary protraction of the case, and the diminution of the symptoms of labour, made me doubt their accuracy.

In consequence of these impressions, I diminished the frequency of my visits; but whenever I went to inquire for her, I always found her in expectation of lying-in without delay, though the precursory symptoms of that state were by no means so severe as they were a month before. Two months had now almost passed since the expiration of the ordinary time of pregnancy, without any greater probability of the accouchement than existed several weeks before. At last the same symptoms which manifested themselves at the close of the ninth month, began to return; she felt precisely in the same manner as she did then. I was sent for; and in two hours after my arrival, and in two months after the expiration of the usual period of pregnancy, effective pains arrived, and produced, to my great satisfaction, a fine and active boy. There was nothing remarkable in the stages into which authors divide the process of labour. They succeeded one another with rapidity; and two hours almost saw the beginning and termination of the first and last.

One circumstance occurred that arrested my attention, and ought to fix that of the medical world, in consequence of the light it sheds on some of the most interesting and practical questions in legal medicine. The membranes did not give way until the head was almost at the extremity of the passage, resting on the perineum, and about to be born. The moment they broke, the infant screamed, and excited so much the amazement of the mother, that she sprung into a sitting posture, and in a frantic, though feeble manner, demanded whence that cry proceeded, and what was the matter. Although scarcely able to suppress my astonishment, I replied, in order to appease her, that the noise which she mistook for the screams of a child, arose from the breaking of the waters. She therefore became calm, resumed her position in the bed, and in a few minutes after a fine strong and active boy was ejected with considerable force, and made the room resound with his cries; and, as if the whole history of its birth were destined to be pregnant with curious and instructive circumstances, the umbilical cord was no sooner divided, and the little urchin rolled in the usual covering, than he placed one of his fingers in his mouth, and began to suck it, with as much eagerness as if it had been the nipple of his mother. I regret peculiar circumstances prevented me from ascertaining his weight. In size he seemed to exceed the ordinary standard of children at birth; and his mother assured me, that in strength and bulk, he was much superior to her other children at the same period.

The celebrated Mauriceau, La Motte and Orfila, have long since observed, that children who remain in the womb longer than the usual time, are much stronger and stouter than those that come forth at the ordinary period. The history of my case affords no inconsiderable illustration of the truth of their observations.

I must now beg leave to mention some circumstances that seem sufficient to illustrate and explain the phenomena of this case. Her husband lost, about the third month of her pregnancy, the situation which furnished the means of subsistence for his family; and, in hopes of procuring another, he continued in this town until his wife's pregnancy was so advanced as to prevent her from accompanying him to another country where employment might have been procured. Thus he was obliged, at considerable inconvenience, to await its issue; and in the mean time to experience, in common with his family, all the privations that necessarily accompany poverty, and which the memory of better times rendered the more acute and melancholy. Her constitution, since the date of her husband's misfortune, was evidently yielding to the pressure of chagrin, ennui, and a train of gloomy and pensive feelings, which the increasing wants of every day naturally created. At last he was obliged to submit to the imperious demands of necessity, and to leave his wife and family without a friend in this town, whilst he endeavoured to find employment in Ireland. This he did with the more reluctance, as he remained with her for some weeks after the expiration of the ninth month of her pregnancy, in expectation that she would every day lie in after the arrival of the ninth month. At length he abandoned her in despair, exclaiming, in the anguish of disappointment, that he believed she would never be brought to bed. It is more easy to conceive than describe the melancholy feelings of his wife, and the depressing passions and affections of the mind, to which her loneliness necessarily subjected her, in consequence of the departure of her husband at so critical a period.

Before I make any observation on the application of these facts to our moral and legal code, it may be necessary to pass rapidly in review the moral causes that influence and direct the functions of our system, and to demonstrate how consistent with the laws and mechanism of labour these extraordinary births are, and then to illustrate the whole by historical facts derived from the most authentic sources.

It is well known that the affections of the mind control the functions of the animal economy; that health and disease are induced or removed in proportion as these moral causes vary in their character. Contrast the effects which contentment, cheerfulness, joy, extasy, and the other gradations of pleasure, produce in regulating our health, with the morbid alterations which grief, chagrin, fear, and the other gloomy and depressing passions cause. In those subject to the influence of the first, the functions of digestion and absorption are active, the circulation is rapid, the countenance becomes animated, the eyes brilliant, and a general impulse is felt to acts of benevolence and generosity. Their ideas, voice and movements, assume an energy and variety to which the others are inadequate: But it happens, in the extreme of these feelings, that the effects are often analogous to those that arise from the gloomy affections of the mind. Every one knows how often extreme joy will excite pain and constriction of the chest, how the heart palpitates, and the respiration becomes interrupted with convulsive sighs, and how involuntary tears are shed; and how, in many instances, the whole system becomes so affected, that syncope, apoplexy, tremors, mania, and even death, frequently arise from this cause. It is said in history, that Sophocles died of pleasure on receiving the prize of Tragedy; that Diogenes expired in the same way, embracing his three sons, conquerors in the Olympic Games; and that Leo the Tenth fell a victim to a similar feeling, on learning news fatal to France. But what function or organ of our system is free from the morbid alterations which ennui, chagrin, grief, and fear excite? Do we not see, in every day's experience, how the circulation and respiration are impaired and retarded by their means, how the appetite and other wants are destroyed or suspended, how nutrition and the whole animal economy change—the visage becoming pale and melancholy, the eyes hollow and dull, digestion languid, respiration imperfect, and a general emaciation seizing the whole frame? Hence the group of nervous diseases that accompany persons victims to grief or disappointment. Melancholy, mania, hysteria, epilepsy, spasms, headachs, abortions and suppressions of the menses and other affections of the uterus, correspond and alternate but too often with these debilitating passions. How often has sickness, which resisted the ordinary means of cure, yielded to the sole influence of the mind reposing confidence in the pretending imposture of some, or the supposed sanctity and efficacy of prayers and good works of others? The celebrated miracles of Prince Hohenlohe afford no ordinary illustration of this. In fact, there is no function of the animal system that is not more or less modified by the operations of the mind. Hence the necessity of studying them, and of removing from the sphere of their action all the moral or other causes that could, directly or indirectly, tend to excite pensive or melancholy feelings. Hence the advantage of inspiring the mind with hope and confidence. It is by these means that amulets, exorcisms, and the greater part of the superstitious practices to which the sick have recourse for recovery, produce such extraordinary effects. Such is the admirable and mysterious connection between mind and body.

But I must, for the sake of illustration, cite a few examples, knowing that facts are more conclusive and impressive than what may appear to many, vague and general reasoning on these matters. I will begin with the fatal effects which the intense and depressing affections of the mind occasionally produce in our system. I have but to refer to the pages of history for examples thereof. There the celebrated Vesalius is said to have died of grief for having opened a man whose heart was palpitating. Clement the VII. is said to have died of the grief, which a letter written by the University of Paris caused him. A similar fate happened to Isocrates on hearing the loss of the battle of Cheroneæ. Racine and Louvois could not survive the misfortunes of Louis the XIV., and Dominique De Vie expired with grief on seeing the place where Henry the Fourth was assassinated. It is unnecessary to swell this paper with more examples. I must now produce some to show, how, through the medium of the mind, the most extraordinary influence is exerted over the functions of the body. Professor Rostan relates the history of a female who wished to be purged, because she was confident she would be cured by that means. Her physician, unwilling to oppose her wishes, but believing that purgative medicine would retard or prevent her recovery, caused pills to be made of bread-crumb, and to be administered to her as strong purgative pills. But however strange it may appear, she had, contrary to his intention, seventeen copious and violent evacuations after having taken a few of them. In the works of Tissot, we find the history of a peasant, who, in dreaming, imagined that a serpent had coiled round his arm, and from that period he was subject to a convulsive movement of the arm. Van Swieten gives a similar instance. The Prince of Saxe Weimar experienced every day at twelve o'clock the first symptoms of the paroxysms of an intermitting fever, which resisted all the ordinary means of cure. Professor Hufeland, his eminent and respectable physician, advanced the clock two hours, and the patient seeing twelve arrive without the return of the paroxysms, supposed himself cured; and in reality was, in consequence of the change which that feeling produced in his constitution. I will cite another example of the power which the affections of the mind possess in deranging or regulating the functions of the body. Bouvart gives the case of one of his own patients, who, from the effect of some commercial embarrassments, was obliged to suspend the payment of his debts. This affected him so much, that a severe disease, which seemed to lead inevitably to the grave, was the result. His benevolent and wealthy physician aware of its origin, drew a bill in his favour for thirty thousand francs, which immediately restored him to his health.

Dr Cullen has recorded the history of a lady subject to epilepsy during four years, experiencing its paroxysms three or four times every day, who recovered from this affection in consequence of the mental shock she received in hearing that her sister was burnt alive. In the writings of Schenkius we find the case of a nun, from every pore of whose body blood flowed, the effect of terror in seeing herself suddenly encompassed with hostile and armed soldiers. I have seen myself a lady fall and continue a victim to epilepsy, in consequence of passing by the respectable mansion of her family, which poverty and law had obliged them to abandon. Professor Rostan cites the example of a schoolmaster, who, when annoyed or excited in any degree by his pupils, was seized with diarrhœa. Why multiply examples? My object is not to enter into any detail of the connection between mind and body. That would induce me to transgress the prescribed limits of this memoir. For illustrations of this inexplicable union, I refer the reader to the works of Boerhaave, Van Swieten, Tissot, Cabanis, &c. Every practitioner in medicine will easily perceive how the whole train of nervous diseases may arise from this source, and will perhaps readily recal to his memory instances of epilepsy, hysteria, spasmodic and other affections of the system in general, occasionally abandoning their victims in proportion as the mind was confident, or the imagination excited by artificial or other means, and *vice versa.*

It is well known that females in general are peculiarly subject to the impressions of the mind. Their sensibility and domestic habits render them susceptible of every gradation of intensity in the consequences of mental affections. Hence we are not to be surprised if the grief into which my patient fell, in consequence of the misfortunes of her husband, should have impaired or retarded the ordinary functions of the womb. To this cause I assign the fact I have mentioned; and the history of other cases, and the observations of every day, justify the inference. But for the sake of argument, supposing it wrong, I will, I trust, demonstrate, that pregnancies continuing ten, eleven, or more months, are consistent with the laws and mechanism of labour, and that the gratuitous assertion of those who deny them altogether, is contrary to the evidence of history and the principles of physiology or medicine.

According to the ordinary laws of pregnancy in our species, the fœtus is expelled at the close of the ninth month; because its size at that period is usually such as to cause that degree of distention and irritation of the uterus which occasions its parietes to contract, and that action to take place in the diaphragm and muscles of the abdomen which is necessary to expel the fœtus. Now, these contractions, it is evident, will depend on the powers of sensation and extension of that organ. For example, in an uterus endowed with much sensibility and little extensibility, it will not be necessary, to produce the phenomena of labour, that the fœtus should acquire that degree of development that would be necessary in one of little sensibility and easy dilatation. Now, this is what precisely takes place in the inflammation of that organ; and hence it is the frequent cause of abortions in females, the contraction being thereby prematurely induced. So that the whole question is reduced to know, whether it be possible, that in an uterus of the ordinary powers of sensibility and extension, the fœtus may be sooner or later in acquiring the volume necessary to induce the contractions required for its expulsion; and to know, whether, without any alteration of the natural growth of the fœtus, the uterus may be endowed with greater or less powers of sensation or extension, either the consequence of some particular disease, or of some peculiarity in its natural properties, sufficient to accelerate or retard parturition. Now we know that some women never go beyond the seventh month, and that their children survive, though generally weaker than those that come into the world at the ordinary period; and supposing the existence of contrary causes, we must naturally admit the occurrence of birth, long after the expiration of the natural time, as in the case of my patient.

We also know that the sensibility of every part, and particularly of the uterus, is increased by disease; for example, by inflammation, while the power of dilating is proportionally diminished. On the contrary, in an uterus that possesses much humidity, and consequently relaxation, the faculty of feeling becomes less, while that of extension becomes greater. This state of the womb is frequently occasioned by general weakness, and often exists in persons subject to grief or other distressing passions. Now, supposing a child born in an uterus of this sort, it is easy to conceive, that though it may grow in the natural way until it has reached the age of nine months; yet, in consequence of the little sensibility of the womb, and its great power of extension, it will continue to grow beyond the ordinary period, more or less, until it has attained that degree of development necessary to excite the sensibility required for the contraction of the uterine and other muscles, and the expulsion of the fœtus. These considerations evidently demonstrate the possibility of a woman being eleven, twelve, or more months in the family way.

I have already intimated how the affections of the mind, and health of the mother, modify the period of birth, the size and constitution of the child. Every day's experience verifies the assertion. Children are occasionally born with the small-pox, venereal disease, palsy, and many die in the womb; and it is absurd to suppose that they could acquire disease without being sick; and consequently, is it not fair to infer that those that are sickly, will grow more feebly and slowly in the uterus, just as they would after birth in similar circumstances? Hence it follows, that the growth, birth, and health of children in the womb, must be subject to the influence of various circumstances; and that, in consequence thereof, they must occasionally deviate from the ordinary laws of nature.

I must now appeal to the records of medicine to show, that facts similar to the case I have narrated have been believed and asserted by the most respectable and competent authorities in every age. It is true, that if facts were alone to depend on the sanction of great names, there is nothing too absurd or ridiculous that may not be authenticated in this manner. But authority, though useful as historical evidence, will never suffice to establish facts in the natural sciences, when inconsistent with the laws and principles of nature, or contrary to the result of experience or observation. For example, Aristotle assures us, that one woman had twenty children in four successive births, five each time. Albucasis mentions that another had seventeen at one birth; but his Grace, Bishop Otho, determined to excel in the miraculous, tells in the most solemn manner, that he baptized one thousand five hundred and fourteen, the offspring of his niece, at one birth. It is not among such men that I am to discover well authenticated facts. Their ignorance of the natural sciences is but too conspicuous in the opinions they have consigned in their writings; and the same reason will make me pass over the authority of Hippocrates, Aristotle, Pliny, Galen, Avicenna and others, who cite many cases of females having been pregnant eleven, twelve, and thirteen months. Their evidence, however, will at least suffice to show how such facts have been stated in every age. I will therefore pass to more genuine sources, for authentic facts, to times when science and observation were expelling ignorance and the influence of names. I will begin with the seventeenth century.

The Parliament of Paris, in 1647, decided the legitimacy of a child born eleven months after the departure of the father to another country.

The University of Heidelberg allowed the legitimacy of a child born at the expiration of thirteen months. *See* Nebel's

" Dissertation sur les Enfans nées à treize mois, " published in the first part of the eighteenth century.

The celebrated Mauriceau, whose work on Midwifery appeared about the year 1688, and La Motte, whose book came out in the beginning of the eighteenth century, have recorded cases in their own practice, in which pregnancy continued twelve or thirteen months. The former states, that there is no fixed limit either for conception or uterine gestation; that they are influenced by many circumstances over which we have no control. The latter, after citing many remarkable cases that fell under his own observation, says, is there any thing more natural than to imagine, that the child who has taken more nourishment, may grow more in nine months, than another in the same time? And he asks, why should the fœtus, that has not acquired the same degree of development, leave the place designed for this purpose, until it has attained the necessary degree of perfection?

Perhaps the most celebrated question of this nature that was ever discussed in the medical world, was that which, in the close of the eighteenth century, called forth the splendid talents and ingenious theories of Petit, Louis and Astruc. It was to ascertain whether the birth of a child, occurring ten months seventeen days after the death of the husband, was legitimate, and consequently entitled to succeed to the father's property, who at his death was eighty years of age, and subject to much bodily infirmity. It is true, they did not believe or assert the legitimacy of this child from the moral and physical considerations which the infirmities and age of the father, and the immoral character of the mother suggest, but from the principles of anatomy, physiology and experience, they showed the possibility of such extraordinary pregnancies.

At Lyons in 1782, Benoite Franquet was unexpectedly delivered of a child of seven months. Three weeks after, she experienced the sensations that appeared to indicate the existence of another, and in five months and sixteen days after the birth of the first, she was delivered of a remarkably strong and healthy child. Now, this infant could not have been conceived after the birth of the other.

We have another instance of preternatural pregnancy in the affair of Catherine Berard, the widow of Francis Chapelet. This took place in 1808, and on the evidence of the most respectable medical men, a child born ten months and eighteen days after the death of the father was declared legitimate; but in an appeal to the court of Grenoble, this decision was set aside by a majority of one only.

Those who deny the existence of pregnancies ever exceeding nine months and some days, seem to forget the historical cases in opposition to their opinion; yet there is certainly nothing in them more contrary to the laws of nature, than the facts which medical writers of the first rank have transmitted in respect to the fœtus becoming occasionally petrified and dry, from some cause we cannot explain, and remaining in that condition five, ten, and fifteen years in the womb, without much, if any inconvenience to the mother. The man who can believe the fact, which I will cite from Buffon's Natural History, may, with equal consistency, credit others not less authentic. That celebrated naturalist mentions, that a woman in Charlestown, South Carolina, was delivered of twins, one white, the other black, in consequence of dividing the pleasures of her bed, on the same day, between a negro and her husband.

In fact, in whatever manner we view the phenomena of pregnancy in our own or other species, we must not reject facts that seem contrary to the ordinary laws, in the supposition that Nature has prescribed fixed and determined limits to the period of gestation. For, on examining the laws and circumstances that regulate or influence the productions of either the vegetable or animal world, we see extraordinary varieties and deviations from the general laws. We know, for example, that not only in the same field, but on the same tree, or stem, fruit and other vegetables arrive with more or less celerity to maturity, and depend, for the promptitude and variety with which they reach or approach perfection, on the difference of climate, soil, or peculiar mode of culture. Examine the same cluster of grapes, and you will find some sour and green, others mellow; some small, others large; besides many other differences in character and quality. Again, pass in review the animal kingdom, and you will find the exceptions to the ordinary laws of pregnancy as extraordinary, and more frequent than perhaps you suppose. Bitches pup usually in ten months, others in a much shorter or longer period. Sheep generally produce their lambs in five months, yet some exceed or fall short of that period. Cows, horses, and other animals, present in the same circumstances similar irregularities, as the experiments and observations of Mr Tessier, which were communicated in 1817 to the Academy of Sciences in Paris, so irrefragably attest. To illustrate the subject of this memoir, I will briefly state their result.

In 575 cows, 21 calved between the 240th and 270th days, average 259; 544 between the 270th and 299th days, average 282; and 10 between the 299th and 321st days, average 306;

average of the whole 282 days; so that, from the shortest to the longest period, there was a difference of 81 days, that is more than one-fourth of the average time.

In 277 mares with foal for the first time, 23 foaled between the 287th and 329th days, average 322 days; 226 between the 329th and 360th days, average 346; and 28 between the 360th and 419th days, average 372; average of the whole 347 days: difference between the extremes 132 days.

In 170 mares which had foaled before, 28 foaled between the 290th and 329th days, average 321; 128 between the 329th and 360th days, average 341; and 14 between the 360th and 377th days, average 370; average of the whole, 341 days; so that, between the shortest and longest period, there was a difference of 97 days, more than one-fourth of the mean term.

In 912 sheep, the mean term of gestation was about 151 days, and the extreme difference only 11 days.

In 25 swine, the extremes were from 109, to 133 days.

In 161 rabbits, the extreme terms of gestation were from 27 to 35 days.

Every one knows that women, though subject to the same general laws of pregnancy, yet vary like the animals I have mentioned, in the time in which they bring forth their young. Some are delivered of healthy children, capable of growing to maturity in six, seven, or nine months, while others require, as I have shown, ten, eleven, or more months, to accomplish the same duty. Hence, it is evident, that much injury may occasionally arise to innocent females and children, from too strict an interpretation of the laws of this country, that limit the period of pregnancy to nine months and ten days. I believe some suppose that in law, a child born in nine months and twenty days, is considered legitimate. But admitting that even ten months were allowed, and that the law was administered in its literal sense, it would still be insufficient to screen, in many instances, the character and property of many from unjust, though legal punishment. If this be the case when the law of England is enforced, what melancholy consequences may follow in the application of the moral and legal code of Scotland, by which a child born six months after the marriage of the mother is considered as legitimate? If circumstances, for example, had obliged the husband of my patient to separate from her eleven months previous to the birth of his child, he would necessarily suppose that she had polluted the sanctity of his bed, and be convinced that the child was the issue of an adulterous amour, and would, in consequence of these impressions, be induced to abandon her and his child, or subject them to the most infamous and unmerited obloquy. But whatever may have been, in the circumstances I suppose, the feelings or conduct of the husband, there is no doubt but she would forfeit her character in the opinion of the world, and that the moral feeling of the country would affix to herself and her offspring the most infamous appellation, and that the law would disqualify the child from succeeding to the inheritance of the father; and thus the most innocent may become the victims of the operation of our moral and legal code. Let me, for the sake of illustration, suppose that she became a widow immediately after conception, and that in one, two, or three months after the death of her husband, she took another, the child of the first would, in public opinion and in law, be considered the issue of the last, and thus entitled to succeed to his property. Thus the law and moral feeling of the country may become the means of the most unnatural robbery and the grossest imposition. That I may not be supposed to conceive imaginary cases of this character, I will cite from the works of the late Mr Hargrave, an eminent advocate, the case of a woman, who, immediately after the death of her first husband, took a second, and a child was born forty weeks and eleven days after this marriage, which in law was held to be the issue of the second husband.

It may be said in objection to the case I have given, as falling under my own observations, that Mrs D. was wrong in dating the period of her conception. But this supposition vanishes on reflecting, that she had every motive to be accurate, the interest and happiness of her family, the melancholy victims of the delay of her accouchement, and that, being the mother of other children, she must be capable of judging of the identity of the signs and feelings of this state, with those she formerly experienced in the same circumstances. But supposing, as is frequently the case, that she became pregnant a few days after the appearance of the catamenia, and that they flowed at the usual period, but ceased to return in the next, she would then evidently date her conception one month later than she ought; and this mistake would render the history of the case the more extraordinary, and corroborate rather than invalidate the fact, as she would in this event be twelve, instead of eleven months, pregnant.

Again, supposing that she became pregnant a few days previous to their return, and that they made their appearance at the ordinary time, she would then, following the usual mode of reckoning these matters, date her conception some days too soon, and consequently there would still be sufficient reason to characterize this as a very preternatural pregnancy.

But setting aside the evidence, or feelings of the mother, which in all cases must be of primary importance, I ask by what means it happens that she had all the symptoms and physical signs, or indications which, according to the evidence and testimony of the most judicious physicians, are conclusive that she was when I saw her, eight months pregnant, and consequently eleven when the child was born? The physical proofs that justify this inference do not rest on her assertions, nor are they in any degree influenced by her statement. Surely she could not possibly possess the power of controlling the operations of Nature, so as to counteract her laws, and to assume all these physical signs of being eight months pregnant, when she was only six. Such a supposition is as monstrous as it is rediculous.

Justice requires that, when pregnancy exceeds the ordinary or legal term, we ought not to presume the illegitimacy of the issue, unless other circumstances warrant this conclusion. The Roman law was liberal in this respect. The decemvirs allowed children born in the tenth month to be legitimate, and the Emperor Adrian admitted the legitimacy of one born eleven months after the death of the husband, as the mother had a good and moral reputation. I may here observe how dangerous it would be to judge in these matters by such a standard, as the same natural causes that would make a virtuous woman eleven months pregnant, would have the same effect on a strumpet. Such considerations may enable a jury to infer a presumptive judgment, but could never be of any weight in estimating the ordinary or physical operations of nature.

From all I have said I deduce one fair and legitimate inference, that the laws and principles of nature cannot be controlled by the opinions of men; and hence, that the natural duration of pregnancy, not only in our own species, but in that of quadrupeds, though generally terminating within certain limit, is however not fixed by any precise or permanent limits, being sometimes much longer or shorter than we could, from any previous knowledge or principles, suppose or anticipate; that our legislation ought to accommodate itself to these deviations and exceptions from the ordinary periods of pregnancy, by allowing more time than it does to establish the claim of legitimacy, and consequently the right of succession in these cases; and that the evidence of medical men, and physical proofs, should be taken in preference to that of men of the world, or the moral considerations of the woman having a good or bad character.

The history of the case I have given, naturally suggests many considerations and observations, on the means which we usually adopt to ascertain whether a child has been born dead or alive. The subject of Infanticide is most important, whether we view it as an interesting part of natural history, or as it regards humanity. The lives and properties of many must necessarily depend, in various instances, on the accuracy or inaccuray of the ways and means by which medical men decide in trials of infanticide, the natural or unnatural death of the infant; and perhaps there is not in the records of medicine a case more calculated to throw light on this subject than the one I have detailed. I will, therefore, reserve for a future occasion the observations and reflections I have to communicate on this point, and which appear to me to arise in the most natural and convincing manner, from the phenomena of the case I have just submitted to the eyes of the profession.

Liverpool, 1825.

II.

A Case of Fungus Hæmatodes, combined with Scrofula. By F. Proudfoot, M. D., Kendal.

Mr D., thirty-one years of age, applied to me about a year ago, complaining of a constant pain and uneasiness in the hypogastric region; and stated, that he had not for some time gone to stool without the previous use of medicine. He laboured under general symptoms of dyspepsia. The treatment was commenced by a course of aperient medicine, with attention to diet, clothing, and exercise, &c.; but it very soon became obvious that the disease was of a character not likely to be cured, or even much relieved, by the aid of medicine. Upon particular inquiry, it appeared that a year before, in descending a stair-case, he had fallen, by which he slightly injured the testicle of the left side. He stated farther, that after some time the testicle became tender, and began to swell, which induced him to apply to a surgeon, who gave him some pills, and directed him to apply cold lotions to the testicle, and subsequently poultices. The swelling of the testicle continuing to increase, it was opened by a lancet, and, according to his account, matter was discharged. On my examining the testicle, it was found considerably enlarged, and a thin serous discharge oozed from the puncture, which had no disposition to heal. The abdomen was hard and tense, though not much enlarged; the tongue was redder than usual, with a white streak in the middle; the pulse was generally upwards of 100; the secretion of urine was natural; but the continual pain and uneasiness in the lower part of the abdomen, and the inability to procure a motion without medicine, were the most prominent symptoms of the disease. He tried the use of an artificial mineral water, along with an alterative course of mercury, but with scarcely any advantage. The hardness of the hypogastric region continuing to increase, he rubbed the ungt. hydrarg. on the abdomen for three or four weeks without any better success.

From the affection of the testicle, it appeared very probable that there was a scrofulous or hæmatoid tumour in the abdomen, impairing and interrupting the natural peristaltic motion of the bowels; but, of course, the precise nature and extent of the disease could not be ascertained. After a trial of the foregoing remedies, and the conjecture formed as to the nature of the disease, iodine was recommended and tried; but it was found that no medicine gave so much relief as the ol. ricini, in quantity sufficient merely to empty the bowels every day, or every second or third day, according to circumstances; and for several months he used scarcely any other remedy. Anodynes, either by the mouth, or used externally in the shape of unguents or liniments, gave no relief from the pain and uneasiness in the lower part of the abdomen. Although the appetite continued pretty good, the body evidently began to waste; and a slight cough, which he was inclined to disregard, and which he maintained was constitutional, began to increase, accompanied with increased expectoration of an aqueous fluid, mixed with mucus, arising apparently from some irritation in the lungs, and unaccompanied with any indication of suppuration having taken place. About the same period, general constitutional disturbance supervened; the pulse was seldom under 110, and often as high as 130 strokes in a minute; and from the progress of the complaint, and the remedies already tried in vain, and all abandoned (with the exception of the ol. ricini), the termination became more and more certain. About this time, frequent inquiries were made relative to the vertebræ of the back, expecting also that matter would in all probability appear at the insertion of the psoæ muscles. In July he became decidedly hectic; the pain, swelling, and tension of the abdomen, very sensibly increased, along with an aggravation of most of the other symptoms; the tongue had a very remarkable red fleshy appearance. At length the abdominal integuments became so tense as to indicate that they would give way. The parietes of the abdomen *separated* at the umbilicus on the 22d of August, accompanied with an enormous discharge of purulent matter, at first of a healthy appearance and mawkish smell; then the discharge altered, and occasionally the discharge was serous, curdy, or ichorous, and again puriform. The swelling, pain and uneasiness in the abdomen, subsided very considerably, the appetite also improved, and the patient flattered himself that he might ultimately recover; but I gave little countenance to these surmises, briefly stating, that a recovery was not impossible. The testicle gradually, though slowly, increased in size, but gave little uneasiness, communicating a soft elastic feeling to the touch; the glands in the left groin became contaminated, and an abscess pointed near the anus, which was opened, and discharged a curdy purulent matter: the opening at the umbilicus enlarged, and the discharge from it still continued variable in appearance and consistence, and very copious; the patient became exceedingly emaciated; aphthæ appeared on the tongue and inside the mouth; and although he had not for nearly two years had a stool without the aid of medicine, a diarrhœa now took place, which he regarded as a favourable symptom. He was admonished accordingly; and after much suffering, he died on the 20th of November.

Leave to examine the abdomen was obtained, upon condition that it was to be as little disfigured as possible. The emaciated state of the body was very strikingly conspicuous. The aperture at the umbilicus was about an inch in diameter, and of an irregular jagged appearance, leading, as it were, to an apparently empty cavity. This impression arose partly, perhaps, from the receding of the abdominal viscera, but chiefly from the partial falling in of the edges of the opening over the cavity of the abscess. On opening the abdomen, the parietes were found remarkably thin; so much so, that scarcely a vestige of muscular fibre was discernible. Near the aperture, however, the parietes were found indurated and thickened from lymphous depositions into the cellular membrane. On laying back the abdominal investment, and attempting to expose the viscera, the appearances were very remarkable, and such as the writer had never before seen, in the examination of several hundreds of dead bodies, at which he had been present. The bowels were not distinguishable but in two or three places, and that only for a very inconsiderable space; so much were they concealed by adventitious membrane, and altered by disease. The bowels were completely agglutinated together, from depositions of a glairy and scrofulous matter among their convolutions, and appeared bound down to the spine by the membrane already alluded to, exhibiting one continuous and apparently solid mass of disease. The abscess from whence the enormous discharge had issued during the last three months of the patient's illness, occupied chiefly the right inguinal and iliac regions, so that the bowels were pushed a little to the left side of the abdomen. The fat usually found in the omentum had become wholly absorbed; indeed scarcely a vestige of the omentum itself was distinguishable, so completely was it incorporated with the general mass of disease. In some places, the layers of coagulable lymph, which, with the omentum, entered into the composition of the adventitious, or diseased membrane, which obscured the bowels from view, and covered them anteriorly, were broken up, and appeared in shreds and patches, or in bands, not unlike cobwebs. On surveying this diseased mass, it seemed very strange that there could remain a passage for the transmission of the feces, and still more wonderful that life could be carried on till such a complete change in appearance and deviation from the natural structure was accomplished. The walls of the abscess were of the genuine scrofulous kind, being lined in different places with a thin smooth delicate membrane, and covering the curdy yellow incrustation which usually forms the wall or boundary of the scrofulous abscess, primarily giving origin to the membrane, and is reckoned peculiarly characteristic of scrofulous disease. Sinuses ran in various directions about the rectum. The aperture at the umbilicus, and that near the anus, were only different openings of the same abscess. The bowels may be mentioned as universally diseased, inflamed, thickened, and ulcerated, having in some places effusions of blood between their coats, giving, on a superficial inspection, a gangrenous appearance. The whole of the contents of the abdomen were studded with numerous round or oval whitish bodies or tubercles, varying, in dimension, from that of a small pea, to the size of a large garden bean. These tubercles or morbid excrescences very generally adhered to the peritoneum, and what might be supposed to be the omentum; and although the liver, stomach, duodenum, and part of the ileum, were in a much healthier state than the other parts contained in the abdomen, the peritoneum covering the liver and stomach was crowded with the same kind of rounded pulpy excrescences. In the substance of the liver, however, we could discover none of these tubercles; indeed this viscus was remarkably healthy. The tubercles or excrescences were of a whitish colour externally, covered by a thin delicate capsule, and when cut into, were found of a soft, pulpy, unctuous, or medullary consistence; many of them very much resembled the substance of the brain.

It is not a very frequent occurrence for an abscess to burst at the umbilicus. The time the patient lived afterwards, and the enormous discharge, accompanied with the admission of air * into the cavity of the abdomen, by the aperture at the umbilicus, may also be considered unusual, and deserving of particular notice. It was doubtless the comparative healthy state of the stomach and upper portion of the small intestines, with the patient's period of life, strict observance of regimen, and due evacuation of the alimentary tube, that mainly enabled him so long to withstand the inroads of the disease. The agglutination of the remaining portion of the intestines, and the manner in which they were covered anteriorly by the adventitious membrane of adhesion, enabled them to resist the stimulus of the external air, so that the same consequences did not follow, as in cases where air is admitted into the abdomen, in the healthy state of the viscera. The abscess has already been characterized as belonging to the scrofulous kind; but there seems reason to doubt if the disease of the testicle, and the rounded medullary excrescences, were precisely of a similar nature. It appears, from the history of the case, that the affection of the testicle arose from an injury, and was the primary, and for three or four months, the only symptom of indisposition. The subsequent symptoms of disease in the abdomen, occurred after a considerable lapse of time, as already stated; and perhaps, it may be inferred, from constitutional contamination. The affection of the abdomen, swellings of the left groin, and brain-like excrescences in the mesentery, and omentum, and adherent to the peritoneum, may be considered as the secondary symptoms of the disease.

Indeed, the whole history of this case is so characteristic of fungus hæmatodes of the testicle, as to warrant the inference of this having been a case of that formidable and incurable malady. The analogy between this disease of the testicle, as described by Mr Wardrop, in his excellent Treatise on Fungus Hæmatodes, and the case now under consideration, is so striking, as scarcely to admit of any doubt as to their identity. In illustration and support of this position, however, I shall briefly advert to them here, referring the reader to the cases in the work itself, for a fuller and better account: in which they are

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* When the abscess was dressed, bubbles of air were emitted from the orifice, with a purling or gurgling noise.

narrated in that perspicuous style which characterizes all the writings of that very eminent surgeon.

The first case is that of a man twenty-eight years of age. He had a swelling of the left side of the scrotum, the size of a goose-egg, which had been forming for nine months, unattended with pain—pulpy and soft, resembling hydrocele. The operation for hydrocele was performed fruitlessly. The testicle was found diseased, and was extirpated. The wound healed in three weeks. About that time the patient was affected with pectoral complaints, accompanied with hæmoptysis, and a tumour the bulk of a large fist formed over the upper lumbar vertebræ. The tumour had a pulsation communicated to it from the aorta. He soon died, with all the symptoms of hectic fever. On examining the testicle after extirpation, it was elastic, though soft, and of a dark brownish colour, having sacs containing a transparent and glairy fluid. "On opening the abdomen after death, the spermatic cord of the affected testicle appeared indurated, and several swelled glands were found adjacent to it, and stretching in a chain to the very large mass of disease, which had been felt during life through the abdominal parietes. This mass seemed to be chiefly composed of a cluster of diseased glands. It was soft when cut into, had a dark-brown colour, and in some parts of it was found a quantity of thickened grumous blood. This tumour surrounded the aorta and vena cava, the coats of the latter being evidently diseased, and its canal apparently somewhat lessened."—"In the liver there were many tubercles, containing a thick blackish fluid. In the pelvis of the kidney, a quantity of purulent matter was found. The lungs were tuberculous, some of the tubercles being also filled with a thick blackish fluid."

The second case was that of a man 25 years of age, who, three years and a half before his death, observed a tumour in the scrotum, the bulk of a small bean. In two years and a half from the appearance of this tumour, the testicle had acquired the bulk of an orange. The testicle and spermatic cord were hard and painful. The complaint was attributed to a syphilitic affection. He used mercury and blisters with little benefit. "At last, after having complained of pain in the belly for several days, a tumour was felt in the right side of the umbilicus; it was of a rounded form; gave a sense of fluctuation when pressed upon; and was painful to the touch. The swelling of the testicle and tumour in the belly continued to enlarge and to give excruciating pain, till at last, after having brought on symptoms of hectic fever, he died." When the testicle was removed, it weighed four pounds; was soft and elastic, and seemed in some parts to contain a fluid. It proved to be spongy and cellular, and composed of lobes varying in appearance; some of them resembled brain, others had a chalky whiteness; some appeared vascular and dark-coloured, and others had cells containing matter like the boiled yolk of egg.

" The tumour in the cavity of the abdomen presented a very extensive mass of disease, surrounding portions of the mesentery and intestines, and involving the large vascular trunks. No very material difference could be perceived in the structure of this tumour from that of the testicle, except that the cells were much larger, the texture more fibrous, and the whole mass much looser. When cut into, it had more of a honeycomb appearance; in some places it felt harder than even the testicle; and in several points, ossification could be distinguished. Two tubercles of a similar nature to the disease in the testicle and abdomen were found in the liver."

A young man, eighteen years of age, had a considerable swelling in the right side of the scrotum, of a uniform shape, and soft to the touch. A ligature was applied to the spermatic vessels, and the testicle dissected out. On the sixth day after the operation, he had pain in his belly; and after one or two hectic paroxysms, he died on the tenth day. " On opening the belly, the spermatic cord was sound within, as well as without, the abdominal ring, but there was a considerable enlargement of the lymphatic glands lying along the fore and lateral parts of the lumbar vertebræ. The glands, when cut into, were of an uniform white colour, and soft consistence, some of them having acquired the bulk of a small fist."

A patient, thirty-three years of age, had had a swelling of the right testicle the bulk of two large fists, and of a very globular form; pain darted along the spermatic cord, and across the loins. On the anterior part of the scrotum, there was an opening from which there issued a copious discharge of purulent matter. This swelling had commenced eight months before, and the operation for hydrocele had been performed by a surgeon unacquainted with the nature of the complaint. The testicle was extirpated, as affording the only hopes of saving this patient's life. He died twelve days after the operation.

" The testicle weighed two pounds; the vaginal coat and albuginea were firmly adhering to one another, and the gland was converted into a soft pulpy mass, in various parts mixed with blood, none of the natural structure remaining. On opening the abdomen, a chain of swelled glands were seen stretching from the internal abdominal ring, to a large diseased mass which was situated on the lumbar vertebræ. This mass seemed to be altogether composed of diseased glands, the whole of which, as well as those along the cord, being converted into a pulpy matter, parts of which were as soft as cream, others being firmer than brain."

The last case I shall allude to, is that of a patient twenty-two years of age, admitted into St Thomas's Hospital. About five months before, he had been attacked with a pain in the testis of the left side. The testis swelled, gradually enlarged, and felt soft and pulpy, yet not sufficiently so as to give the sense of fluctuation. Seven weeks previous to his admission into hospital, a tumour was observed in the abdomen, on the same side of the umbilicus. The swelling of the testis and the tumour continued to increase; and after much suffering, hectic fever came on, and he died about ten weeks from the time he was admitted into hospital.

"On making an incision into the enlarged testicle, a pulpy mass appeared, composed of broken coagulable lymph, and of a blood-coloured serum. The absorbents of the spermatic cord were very considerably enlarged, their coats thickened, and small tumours appeared at irregular distances."—"The small glands on the loins, which receive the absorbents of the testicle and cord, by their enlargement and union, formed a tumour on the lumbar vertebræ, weighing nine pounds and a half."

"On opening the tumour, it exhibited a similar appearance to the testicle, and there could be no doubt of the disease being of the same nature." The receptaculum chyli and thoracic duct contained a similar matter, and the latter was lost opposite to the curvature of the aorta, "in a swelling as large as a moderate sized walnut, differing in no respect excepting in bulk, from the swelling in the abdomen."

The analogy between those avowed cases of fungus hæmatodes of the testicle, and of that now under consideration, appears to be so manifest, as to justify the parallel that has been drawn, and to leave no ambiguity as to the nature of the disease of which the patient died.*

It has been already stated, that the disease under which this patient laboured was at one time supposed to have been of a scrofulous nature; as the abscess, as well as some of the depositions among the convolutions of the bowels, very much resembled the effects of scrofulous disease, and seemed to verify the accuracy of the remark. The analogy between some forms of scrofula and fungus hæmatodes is considerable. Laennec, Wardrop and others, state, that they may coexist. It seems to have been the case in this instance. It will readily be conceded, that it may occasionally be extremely difficult, if not impossible, to distinguish a scrofulous affection of the testicle from the soft pulpy tumour constituting the primary symptom of fungus hæmatodes of that organ. The history and progress of the two diseases, however, are widely different, although they are both diseases of early life. Scrofula often admits of cure, or spontaneously disappears. It is a hereditary, and consequently, strictly speaking, a constitutional disease, generally attacking the lymphatic glands. Scrofulous affections thus appear in different parts of the body at the same time, not by constitutional contamination from a primary local affection, as in syphilis, cancer, fungus hæmatodes, &c. &c. but from a particular and original diathesis.

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* It is regretted exceedingly that the testicle was not examined, but leave to inspect the abdomen was obtained with difficulty; and any farther interference was decidedly objected to. When, therefore, the examination of the testicle was rendered of so much interest by the pulpy, brain-like excrescences in the abdomen, the only relation of the deceased had purposely (I believe) gone out of the way, so that the examination of the testicle would have been a direct breach of a specific engagement.

It is also probable that the same kind of pulpy tubercles would have been found in the chest, as the pectoral irritation increased with the duration of the disease.

But, independent of the circumstances now mentioned, there are also distinctive marks in the local affections, and in the morbid masses themselves, which greatly assist us in our diagnosis. Primary hæmatoid tumours have not the same tendency to arise in lymphatic glands,—a circumstance that almost always occurs in scrofulous affections. The hæmatoid tumours usually enlarge, ulcerate, and shoot out a fungus, which discharges a thin fetid sanies, often mixed with blood. They are intersected with bands, and lobulated, varying in appearance and consistence, and more complicated in their structure than the secondarily affected glands or tubercles. Both generally retain the pulpy medullary structure; but the consistence and aspect of the glands are more uniform.

When a scrofulous gland suppurates, it generally contains a yellow or green-coloured matter; by and by, the matter changes, becomes flaky, curdy, or serous; the abscess is very generally circumscribed, or bounded, after some time, with a yellow incrustation, lined with a delicate secreting membrane. In other instances, scrofulous glands have been found enlarged, from depositions of coagulable lymph; and in many cases the glands have a caseous consistence, mixed with a calcareous substance, composed principally of phosphate of lime. Scrofulous masses of a white soft granulated matter, as thick as the edge of the hand, have been found behind the peritoneum; and the mesentery has also been found converted into a cake of this substance.

In cases of fungus hæmatodes of the testicle, extirpation of the diseased organ is the only curative means promising to be of use; but unfortunately this step is not likely either to be recommended or acceded to in time to prevent constitutional contamination, from the little pain and uneasiness the tumour generally gives to the patient, and the doubt and uncertainty, in many instances, as to the precise nature of the disease, on the part of the medical attendant. When the constitution has once become contaminated, which may be suspected by a thickening of the spermatic cord, shooting pains, or the appearance of tumours in the abdomen, farther than the alleviation of urgent symptoms, human aid has hitherto proved of no avail.

Kendal, December, 1825.

III.

Psychological and Medical Researches respecting the Cures attributed to the Prayers of Prince Hohenlohe. By D. C. Pfeufer. *(Continued from p. 73 of the preceding Number.)*

III. Attempt to explain the operation of the process used by Prince Hohenlohe.

The instant cure of the patients above mentioned, who were, in our town, the first subjects of Prince Hohenlohe's religious therapeutical experiments, acted on the public mind like an electric shock, and every one hastened, big with expectation, to the highly favoured individual, to ascertain, with their own eyes, the marvellous event. I myself was extremely gratified to see together, at the window, the two sisters whose idea had been for years associated in my mind with the bed of suffering, and to hear them, with serene and thankful countenances, and with their own lips, declare their deliverance to the crowd that rushed towards them. I had but the single wish, that their comfortable feelings might be permanent. While I pressed through the crowd, I remarked many faces pale with ecstacy and wonder, and many eyes swimming with tears.

One must have seen something of this kind, to be able to form a conception of the effect which those curative trials produce in patients, who are abandoned by art in despair, and consigned victims to a wretched fate. One must be acquainted with the various views that are taken of those trials, to understand how, in a few hours, the majority of the inhabitants of our town were set in motion by them, and not so much as a single family was to be found in which they were not occasions of joy, wonder, or silent doubt. The reflecting part looked for certain information to the official investigation of the facts, and, above all, to the future, which was to determine whether this treatment would be confirmed, and whether the healthful sensations of the patients would prove permanent. The mass, however, and, amongst the rest, some men of education of all ranks, gave full credit to every report, and interpreted those marvellous events into calls from Providence to make men better, and lead them back to the old fallen faith.

Now that the impulse was given by those successful experiments, to which were added in the promulgation a thousand interesting circumstances *(fama crescit eundo)*, nothing could destroy the powerful impression to which they gave rise, far and near. As early as the evening of next day, all the streets, inns and churches, were filled with sick from places at various distances; and it was to be expected that the concourse would be greater in proportion, as the instances of success hourly announced should be more striking. Until a late hour the Prince's humanity was put in requisition, and every one looked forward to the coming events, big with expectation. From the first dawn of the morning, the street before the Prince's lodging was beset by an immense crowd of sick. A stranger who, ignorant of the cause, should have entered the street, might readily have supposed that he had wandered into a general hospital of an entire nation.

In the preceding remarks, the explanation of the operation of these experiments is partly contained. The mind becomes at length powerfully affected, and the soul works on the body, by means of its close connection with it through the nervous system. This intimate alliance between soul and body is often disturbed, nay, even interrupted, but never during life altogether dissolved. Every observant physician must be convinced, that in many diseases this reaction is greatly increased, and that, under such circumstances, the sentient being exercises a more extensive influence over the body than in the state of health.

In the so called evangelical diseases, this seems to be the most remarkable feature; and it is such diseases, accordingly, that Prince Hohenlohe, by his own declaration, finds most suitable for his purpose. They are for the most part the consequences of functional disorder of the nervous system, in which the solar plexus, and the sympathetic nerve, act important parts.

The former has, according to Reil, a decided influence on the temperament of man, on his feelings, and on his senses, soul, and moral habitudes. Now, nothing affects the nervous system of the epigastric region so powerfully as religious emotions; it is through the medium of the nervous system, therefore, principally, it would seem, that Prince Hohenlohe's method operates.

It is unnecessary to mention the great heroes of the early Christian Church, who, through devotional feelings, were enabled to endure the most ingenious tortures, and even death itself. Neither need I refer to the wonderful deeds which the like feelings called forth during the crusades. Every physician has known instances, in which religious emotions, awakened by the holy exhortations of a well informed priest, have instantly reinspirited a desponding patient, and eased him of the acutest pains. To myself two most striking instances have occurred, in which the instant alleviation could only be attributed to religious emotion. The families in which these instances occurred are yet alive, so that I can easily point them out, which delicacy forbids me to do here, to any one that may doubt my word.

Baron S., who had suffered for many years from a painful, incurable complaint of the bladder, fell, after a fortnight's confinement to bed, unexpectedly into such a state of weakness, that, though quite sensible, he yet could neither move nor speak, and every moment his last struggle was looked for. With constantly increasing anxiety, I, and those that stood around, awaited the coming of the priest, who lived about 1½ hour's walk distant. This gentleman, who was distinguished for information, inspired the exhausted patient with courage, and administered, after a sanctifying exhortation, the sacrament and the inunction, according to Catholic usage and the German ritual. During the prayer of the priest, the patient was observed to raise his eyes unusually often. Immediately on the completion of the holy ceremony, he raised himself up in bed without any assistance. A breathing perspiration now warmed his body, which was previously for the most part cold; the pulse, from being trembling and irregular, became stronger and regular; and the patient felt himself so much strengthened, that, with the completest self-possession, he dictated to the priest his last will. He found particular difficulty in describing to me the feeling that darted through him, at the words of the prayer, "Lord, take this cup from me; but not mine, but thy will be done." The patient retained his strength and reason for three days after, and until within a few hours of his dissolution.

G. H., a countryman, had been for several years unable, unless occasionally, to leave his bed, on account of gout, and when he got up, he could not move without crutches. Being 77 years old, he had now but one wish remaining, which was to see his son, who had studied theology, offering up the sacrifice of the mass at the altar of his parish church. Two years ago he had his wish fulfilled. The instant he was informed of his son's arrival, he left his bed, went without crutch or support to the church, spent two hours there, and afterwards walked about the village without assistance, which he does still to this hour.

Such observations are however the less strange, since similar results have been obtained from the operation of the mind on the nervous system, without any excitation of exalted religious feelings. What marked influence do the passions exercise over the functions of several of our organs? How powerfully do music and poetry operate? He that suffers from severe toothach, feels himself often instantly relieved from pain the moment the dentist enters the room. The mother whom grief for the supposed death of her only son has brought to the brink of the grave, is instantly restored to health on receiving authentic information that he yet lives. I knew a girl in the vicinity of Wurzburg, who, after being deaf for several years, instantly regained her hearing on being made acquainted with the sudden death of her father.

In Nostalgia there is an inconceivable insensibility to every thing around us, yet there are no prominent morbid appearances; the unhappy creature becomes the sure prey of death, if his longing be not satisfied; the hour of his return to his native country strikes, and new life glows through his frame.

But let us examine the operations and practice of the physician himself. Without the aid of a noble and more intimate relation, like that between the magnetiser and his patient; without the skill to adapt himself to the character of the sick, the physician will effect but little with all the medicinal treasures of the old and new world. Hence says Walther (Marcus Ephemeriden der Heilkunde, 4. B. 3. H.) the physician is, after the poet and artist, the third son of Apollo, and begotten only under the influence of mild constellations. He that depends on medicine alone, as such, seeks corn in thrashed straw. Medicine is but an instrument of cure; but the physician heals, not by medicine alone, but also by the glance of the eye and by the animating word.

A cure also often depends on something more elevated and intellectual, and in so far marvellous that the principle of it lies out of the sphere of ordinary thought and of sensible perception. In this class are comprehended the religious experiments of Prince Hohenlohe. Their efficacy depends entirely on the influence of the mind over the body; in other words, on an agency purely psychological.

On taking into consideration all the circumstances under which these experiments came into notice, there appears no doubt, and every one must be convinced, that they can seldom fail of their effect, (viz. tranquillization of the afflicted spirit, and establishment of its dominion over morbid feelings), when undertaken with proper precaution, and with a certain pomp and dignity.

A priest of princely race, full of zeal in his vocation, not a gloomy ascetic, but a polished and sociable individual, unassuming and modest in his deportment, announces, with unusual confidence, a new method of curing diseases, in two towns in which former ecclesiastical princes had in general governed very mildly. His mode of cure seizes upon the strongest feelings of man, viz. his religious sentiments; he heals sick of all sorts instantaneously, without interested motives, without expense.— He begins his career by the cure of a princess, who, for many years deprived of hearing, had now reduced the profession to its last resources, and who, being related to many great families, must of course excite a lively and universal interest.

To this we must add the character of our age, which is so little consistent with itself, that there seems to be scarcely any thing fixed in terrestrial affairs. Great and astonishing events, constant vicissitudes of starting into being and passing away, of establishing and of destroying, incessantly presenting themselves to our observation, elevate us beyond the narrow bounds of daily life, whose impressions we no longer feel, and whose tameness does not satisfy our imagination or our hearts. We wander, therefore, with willingness into the magic domain of fancy, and surrender ourselves to her dominion with so much the more confidence, as she renders our gains as pleasant as they are easy. Nothing so powerfully excites the fancy as the extraordinary and inconceivable. The educated person, indeed, does not willingly surrender his reason captive; but he still does not swim against the stream, by which, in fact, he is readily carried away. If, in addition to all this, the extraordinary presents itself in a noble and religious form, then it is no wonder if, like an electric stroke, it gives a shock to a great number of sick, who, with eyes shut, threw themselves upon the last profferer of aid, and imagine that, by his means, they have obtained their deliverance from long suffering.

Moreover, when we reflect on the eventful period lately past, it appears very clear, that the general mind must have been prepared for the reception of every belief, working on religious feelings. During a stormy time, virtuous sentiments were more and more thrown into the back ground, or were kept in constant conflict with selfish passions. Amidst those tempests arose a tendency towards the customs of the "good old times," which cannot be mistaken; to those comfortable usages of religion, which, although not torn up from their original ever fruitful soil, during the powerful struggles, were notwithstanding dragged into the whirlpool that embroiled all terrestrial affairs.

Let us then imagine thousands and thousands of human beings under such circumstances, to each of whom every day brings new sufferings, new despondence, and who have but one wish remaining—release by death from their nameless pangs. Electrified by the news that, at Bamberg and Würzburg, there is help and salvation for them through the grace of God, a new life glows within them, and already they feel the term of their misery. With the same longing as that of the leper for the pool of Bethesda, they hurry to the place where all grief vanishes, and where the stream of health and deliverance flows. In ever increasing expectation, and with breasts full of hope, wholly occupied in anticipating the enjoyment of re-established health, dead to every other thought, they approach the scene of healing grace. At every step are announced to them the great wonders that take place there every minute. Scarcely arrived at the place of deliverance, they throw themselves at the foot of the altar, confess their sins, and, reconciled and united with God, hasten to the chosen Priest: All the streets they find filled; with great trouble, and anxious palpitation, they reach the desired residence. Some one carries on his back an old woman into the house. She has been, says report, from her childhood, crooked, and unable to stand or walk. A few minutes pass by, the door of the Prince's dwelling opens, and the same woman walks without support out of the house, thanking and praising the Lord God. Every one presses round her; each one will, with his own eyes, and from herself, learn what has happened to her. Man lives for the present time, of the past nobody has time to think; no one, therefore, concerns himself about the former situation of the person cured. Often, also, he who is not lucky enough to see or speak with her, still asserts, in order to please the crowd, and to appear of some consequence, that, with his own eyes and ears, he has convinced himself. For who is not gratified in being the bearer of wonderful news? And now, in an incredibly short time, the new miracle spreads through town and country; each narrator vies with the other. If the cured person limped at first, he is soon dead lame; another soon after sees him struggling with death; and the story becomes more interesting, and the narrator more consequential in proportion as he is more skilful in tale-telling. *

Each new report treading on the heels of the foregoing, electrifies the nervous system of our patients anew. Already they imagine themselves saved, released from all suffering, and become subjects of the talk of the day. In this downright earthquake of the soul, they spend hours and days, and yet their good star does not rise. At length the report spreads abroad, that at a certain hour and place, the curative experiments will be performed. Each wishes to be the first to derive benefit, and to publish it. Regardless of incivility and rudeness, our patients arrive with hearts full of hope, at the place appointed. They find the princely Priest in an open place, surrounded by a crowd of persons of all ranks, and of all degrees of information. All eyes are fixed on him, and nothing breaks through the solemn silence, unless the resounding annunciation of happily effected cures. They approach the princely Priest not as a man, but as a chosen one of the Lord, to whom God has vouchsafed extraordinary grace. The act of cure is regarded as something exalted, something godly; one thinks himself nearer to God, and a sensation, before unknown, thrills through the soul. I have seen men and women of no common education weep with humility and tremulous emotion; and often did I hear the exclamation, "He is a living Apostle!" In such a frame of mind, no man inquires whether his disease be curable or not. If the patient, after the ceremonies, feels no remarkable alleviation, he tries as much as possible to deceive himself in respect to his situation, and to crush rising doubts, in order that, on the one hand, he may not feel too keenly the pang of disappointed hope; and, on the other hand, that he may not, in the eyes of the multitude, appear a person unworthy of the grace and mercy of God.

To increase the unconditional reliance, which is the main spring of the Hohenlohe process of cure, there is still another circumstance, namely, that it is attended with no cost. If circumstances would allow of physicians treating all patients without expense for attendance or medicine, the world would soon resound with their marvellous cures, and many a patient would

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* A fact like that above really happened. Upon close inquiry, however, it appeared, that the person cured had only limped, but always had the power of walking; the man had taken her on his back, merely that he might be able to press through the crowd with more ease and safety.

then regard his physician as an ambassador from the Lord, who now entertains no extravagant opinion of his sanctity. There is no expense which men more heartily dislike, than that incurred in the restoration of their health; and even should cures bordering on the marvellous be effected by physicians, the expenses connected with them would so occupy the mind as to leave in it no room for wonder.

Dr Pfeufer then discusses the propriety of subjecting Prince Hohenlohe's therapeutical proceedings to the control and inspection of the police. He mentions that the minister for the Home Department established a regulation, that each sick person who wished to try the devotional process of cure, should provide himself with certificates from the parish priest and from a physician, specifying the nature and duration of his complaint, and that the experiments should be made in presence of police and medical officers of the town, and not permitted in the open streets. This regulation Dr P. defends as necessary to prevent dangerous delusions. He gives as an instance of delusion, the case of a child belonging to an excise officer of Bamberg, who was blessed by the Prince; the universal cry was, " There is another child restored to sight!" On inquiry, it appeared, that the child had never been blind, but had merely a scrofulous sore on the hand. He subjoins the following observations:—These experiments should be confined to ecclesiastical functionaries, and not practised as an art distinct from the clerical office. All the prayers and ceremonies of the Christian Church have in general for their object to encourage and strengthen man, to confirm his belief in God, to enable him to overcome morbid feelings and endure bodily suffering. For this reason, great care should be taken that the experiments in question should be performed with the greatest dignity and discretion. The holiest exercises require a kind of display of pomp and silence, by which their splendour and efficiency are increased and exalted. This idea has, in the Christian, that is the Catholic Church, been reduced to practice; hence the great influence of her worship over the soul, and the warmth of the emotions that flow in the breast during her solemn services.

As the anointed minister of the altar approaches the sick, clad in peculiar vestments, and, with holy humility and silence, prays for him with high and solemn dignity, and anoints him with consecrated oil; for the same reason must every experiment undertaken to cure disease, by exciting religious emotions, although not strictly belonging to the sacerdotal office, be performed with becoming pomp and silence; for this can be effected only by the desired profound impression on the soul.

The author concludes his paper with remarks respecting the influence of the Hohenlohe method of treatment on medicine and the medical profession, remarks of no interest out of Roman Catholic Germany.

Note by Dr Clendinning.

Mr Editor,—There are two lines of argument, either of which may be employed against the admirers of Prince Hohenlohe. The first direct: His reputed miracles may be compared with those generally admitted as performed by the Apostles. The second indirect: The effects of his prayers, with their antecedent and concomitant circumstances, may be explained on principles derived from experience, and involving nothing marvellous. The first mode of argument Dr Pfeufer has not touched on, and perhaps not without good reason; the meaning of the word *miracle* is hard to fix; the idea it represents is popular and vague, not scientific and precise; it has, therefore, eluded every attempt to imprison it in a definition. For the same reason, it would be next to impossible to make any enumeration of its elements, in other words, any description which any two disputants would altogether admit. But without a definition of the term or description of the thing, there could be no discussion, at least none ending in a decisive result. Owing to the uncertain import of the word miracle, it is plain the Hohenloheist who would confound, would have an immense advantage over the Antihohenloheist who would distinguish.

Confining himself to the second or indirect line of argument, Dr Pfeufer contends, that Prince Hohenlohe's cures are not to be accounted miraculous, but referred to the influence of the emotions and passions on the body.

There are diseases whose essential elements are organic from their commencement. There are also diseases whose principal conditions are inorganic or hyperorganic, at least at their commencement; and there are proteiform diseases partaking of the nature of both of these. In these last, the affections of the sensitive being are often out of all proportion great, as compared with any sensible organic degenerations found in connection with them. In the first class, which comprehends mutilations, inflammatory, febrile, and chronic organic diseases, the thinking and sentient being is only sympathetically or secondarily affected. Of this class Dr Pfeufer has not known a single case cured by Prince Hohenlohe, very few even momentarily relieved.* Now, it is over diseases of this class that the mind might, previously to trial, be pronounced least capable of exerting any curative influence. It is equally evident, that to Divine power all diseases are alike easy of cure.

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* Have not hæmorrhagies been arrested by sudden fear?

Under the second and third heads fall multifarious morbid affections, popularly known by the names Nervous, Hysterical, Bilious, Hypochondriacal, &c. diseases; in many of those there is no doubt organic degeneration—in many only functional derangement—in some probably no bodily ailment whatever. The influence of emotion over those complaints is well known. Obstinate spasmodic affections have been instantaneously arrested by fright. To a gouty patient, fright has in a moment restored agility; to an hysteric woman tranquillity; to a drunkard self-possession, &c. &c.;—so much for the single passion, Fear.

Of diseases of the third class, connected with no organic vitiation, as effect with cause, it is easily understood how even radical cures may be effected by strong mental emotions.

IV.

Account of a Species of Cynanche Laryngea, *which prevailed at Kelso, and proved fatal to a number of the younger part of the community.* By William Robertson, one of the Surgeons to the Kelso Dispensary.

This disease made its appearance about the latter end of May. At that time the weather was very variable; there were a few warm days succeeded by dry, cold winds from the east, alternating with showers of rain. Numbers of both young and old were attacked with catarrhal affections, and some grown up people whom I saw, with *cynanche tonsillaris,* all perhaps originating from the same cause. The catarrhal affections were got the better of by attention to diet, keeping warm, and giving emetics, sudorifics, and laxatives, according as the urgency of symptoms required. But among those catarrhal affections there appeared a disease of a more formidable nature. The marks by which it was distinguished from the prevailing catarrhal ailments, were its commencing with a hoarseness, accompanied with a shrill half kind of cough, as if the patient wished to force some irritating matter through the epiglottis. At this period there often appeared very little constitutional disturbance, the pulse very little accelerated, seldom exceeding a hundred. The patients complained of a stiffness in the neck, and when asked where they felt most uneasiness, they generally referred it to the top of the larynx, opposite to the glottis. When the fauces were inspected at this early period, if a satisfactory view could be obtained, nothing particular was observed, except a preternatural redness in the tonsils, *velum pendulum palati*, and as far back as could be seen. I saw three in this stage of the complaint; the children were from seven to ten years old. They had all been wading the water late in the evening, and were all seized with the same complaint early in the following morning. It is probable that the inhaling the damp vapour from the water contributed to its production. If prompt and decisive measures were not adopted at this period, and the disease arrested in its progress, it hurried rapidly on to a fatal termination. And this melancholy issue happened too frequently; for at the commencement of the epidemic, parents, not aware of the danger, considering it to be a common cold, and treating it as such, did not apply for medical aid, till the disease had made such progress, that it was out of the power of medicine to afford relief. A number of deaths in consequence happened, which spread the alarm among the inhabitants, and afterwards made them apply for medical assistance on its first attack.

In those in whom the disease had been allowed to go on for the space of three or four days before applying for advice; on inspecting the fauces, when a proper view could be got, the *uvula*, tonsils, *velum pendulum palati* and *œsophagus*, were generally observed to be more or less covered with a buff-coloured coat, and the throat filled with froth and a ropy kind of phlegm, which the patients were constantly endeavouring to get rid off by a forced kind of cough. At this period, the throwing into the fauces, by means of a syringe, injections made of the decoction of the conserve of roses, sufficiently acidulated with the muriatic acid, afforded a temporary relief. What was rather singular, most of those affected took a moderate proportion of food, and seemed to swallow it without great difficulty or pain. Matters often went on in this way for the space of a few days. I saw one patient who lingered out to the ninth day from the first attack.

Towards the end of the disease, all the symptoms became gradually aggravated; the cough and efforts made to expectorate were more urgent, respiration more difficult, the pulse greatly accelerated, with great constitutional disturbance. You might see the sufferers writhing their heads in various directions, with their mouths open, so as to catch the fresh air and prevent immediate suffocation, and continue in this miserable state, until death put an end to their sufferings.

This disease continued to prevail till about the end of June when it disappeared; but the catarrhal affections among the children still continued, though changed into a milder form.

As to the mode of treatment, having learned by experience the fatal tendency of the disease when accompanied by the threatening symptoms already enumerated, I resolved, when called upon at its first attack, to have recourse to the most active and vigorous measures. At my first visit, when the patient was so far advanced in years, and required the loss of a good quantity of blood, if the jugular vein could be rendered so conspicuous as to be operated upon; to prevent loss of time, I opened it, and extracted if possible a cupful of blood. But it often happened that the patients became frighted and restless, twisting their heads in various directions so as to stop the flow of the blood; then to be sure of extracting a sufficient quantity, I ordered two or more good leeches, according to the age and strength of the patient, to be applied to the top of the larynx, opposite to the part they complained of, and allowed them to bleed freely. This was succeeded by an emetic of a solution of the tartarized antimony, which generally brought up a quantity of bilious matter. The patient was next put into the warm bath of a moderate temperature, and allowed to remain in it for the space of ten or fifteen minutes, and then dried and put to bed. To open the bowels, a laxative, composed of calomel and jalap, proportioned to the age of the patient, was given, and the steam of warm water was ordered to be frequently inhaled. If the violence of the symptoms did not seem to abate, a blister was applied across the throat, from ear to ear, and one of the following powders was directed to be given.

> ℞ *Submur. hydrarg.* ℈*ij. Sacch. alb.* ℈*j. Antimon. tartarizat. gr. v. tere simul bene, et divid. in pulv. equal. No. xx. Cap.* 1, *om.* 2*da. hor.*

The strength of these powders was either increased or diminished in proportion to the age and constitution of the patient. After these powders had been used for some time, there uniformly followed a discharge from the bowels of a greenish-coloured matter, somewhat resembling chopped spinage. The first signs of amendment were indicated by the clearing of the voice, and the shrillness of the cough being changed into one more of a catarrhal nature, attended with less difficulty of breathing and more copious expectoration. The expectoration continued for a few days, when the symptoms usually disappeared altogether.

The above may, to some, seem to have been a severe mode of treatment; some of the patients might have got well without recourse to such active measures; but, owing to my former experience of its fatality, I thought it more advisable to subject some to a discipline perhaps unnecessary, than suffer the disease in any to pass into that stage, when neither applications nor medicines of any kind, possessed the least control over it.

The diet directed to be given during their illness, was arrowroot, gruel, breadberry, &c.

I was happy to observe, that after adopting the above plan of treatment, I did not lose a single patient.

I regretted that I had not the opportunity of a *post mortem* examination; but it was evident, from the progress and symptoms of the disease, that the sudden transition from extreme heat to cold, accompanied by moisture, had the effect of producing inflammation in the mucous membrane that lines the glottis, epiglottis, and trachea, which, if not early subdued, produced death by suffocation, from the air passages being obstructed by the secretion of mucus and pus from the surface of the inflamed membrane.

Kelso.

V.

Case of Malignant Cancer, in which Iodine was administered with powerful effects. By George Nesse Hill, Surgeon, Chester.

Extensive as are the objects of medical and surgical daily concern, none rank higher in importance than the treatment of scirrhus and cancer. Zeal in the cause of suffering humanity never appears more solicitous to obtain practical success, than in these truly melancholy destroyers of all ease of body and quietude of mind. Ages have rolled on witnessing the fruitless attempts to remove the first, and cure the last, so that the practitioner of any denomination who has but a proposal to make which carries with it even a mere probability of success, in ameliorating the condition of the sufferers by these cruel maladies, is almost as much entitled to attention as he who, in less stubborn evils, has met with a new remedy decidedly successful in establishing a cure. Perhaps it may with truth be said, that no disease has produced such a number of pretenders to cure as cancer, while in no department has there been less actual success.

The subsequent detail has no higher claims on the public attention than that of affording another proof of the powerful effects of Iodine on the human glandular system, and that it is a new remedy from the poisonous class meriting farther and closer attention than it appears yet to have received. Three or four notices have appeared in the periodical publications from respectable writers on the subject, of the very powerful effects of iodine on the female breast. By a free and constant use of the remedy, complete absorption of the whole organ has ensued, so as to leave scarcely the vestige even of a nipple. The last account I have seen of this kind will be found in the Med. and Phys. Journal for May last, p. 437, by Hufeland. Subsequently one of the foreign Journals reports, that Dr Edward Graafe prescribed an ungt. containing ʒi. of the hydriod. potass. to ℥ij. of pure lard, from which the like effects followed.

December 20, 1822.—I was requested to examine the right breast of a widow, aged 62, of a shattered constitution, having led a laborious life, and been the mother of ten children, exclusive of abortions. The breast presented a large, hollow, fetid ulcer, with ragged thickened hard edges; the subjacent ribs bare, but not denuded of their *periosteum*. Her general health was deranged, appetite bad, with frequent cough; her situation in life in every respect unfavourable. I directed a carrot poultice to be applied, and renewed every five or six hours. Medicines to be taken to regulate the bowels; and when the evacuations assumed a tolerably healthy appearance, the Fowlerian solution was commenced; and happening to agree with the stomach (by which expression I mean to be understood that this organ admitted of its use being pushed to 15 drops twice and thrice *per diem*), in two months amendment was visible. Her general health was improved. The ulcer became less offensive. A weak mixture of the solution and pure water was applied with lint, and over that the carrot cataplasm. In short, at the end of eight months, the general health was so improved, and the appearance of the ulcer such as to justify the trial of Mr Young's mode of cure by compression. Just at this juncture, an accidental visitor from London was at my house, who had been successfully treated by this method. She spoke highly of it, said her breast had been condemned to be removed by some eminent surgeon; but as she was unwilling to suffer the operation, she applied to Mr Y., and was now free from every vestige of the disease. Having procured the apparatus recommended by Mr Y., it was applied conformably to the rules laid down by that gentleman, with steady diligence, and borne with equal patience, until, in the course of three months, the edges of the sore assumed the appearance of cicatrization; but the whole surrounding parts, and particularly the axillary glands, became so painful, and a provoking discoloration in the centre of the ulcer (from which spot she described the pain as all emanating), compelled us to lay aside the pressure apparatus as incompatible in this case. The unhealthy spot at the fundus of the ulcer now rapidly enlarged, and threw out a large tubercle of a livid appearance. This diminished and looked better under the use of the *sulphas cupri.* The surrounding parts were dressed with the *pulv. cretæ ppt.* according to Mr Kentish's mode; the cinchona and opium were administered; and, at the close of the first year of my attendance, the ulcer had assumed a favourable aspect. Spots of new cicatrization were once more visible, but their next door neighbours were the hard ill looking tubercles, soon accompanied by incurvated edges, and a general tendency to bleed from small nipple-like eminences of the fungoid kind. Dry lint imbued with flour and *sp. tereb.* generally succeeded in stopping the blood. The bolar earth with alum never failed. The longest diameter of the ulcer now measured six inches; the general health still good, a little cough excepted. Thus we went on to the end of a second year, without gaining or losing much ground, except that the insulated spots of cicatrization which had appeared under the use of the chalk were no longer visible; the discharge from the whole corroded surface was copious and gleety.

At this juncture, having put to the test every local application and internal remedy I could think of, the powerful effects of Iodine were suggested to me by a very ingenious friend and chemical artist of this city, as worthy of trial in all desperate cases of tumour and vitiated ulcer; I gladly caught at the hint, and applied an unguent consisting of ℈i. of the hydroidate to ℥i. of the purest *axung. porcin.;* this was the dressing twice a-day; it not only gave no pain, but, as my patient observed, "felt comfortable." In a few days, the quantity of discharge was diminished, and its quality improved, the whole surface of the sore assuming a kindlier aspect; the ill-coloured tubercles softened and gradually sloughed away, recalling to mind the applications of our forefathers, designated detergents. In a few weeks, the whole surface wore the most flattering appearance possible. Any surgeon whose eye had then been directed to this poor sufferer's breast, would have said, "Here we have a state of fine healthy granulation;" in truth nothing could look better, or afford a more healthy discharge; all the surrounding hardness was softened and ultimately removed, as though there never had existed any breast. The left breast underwent a similar process; there is not the vestige of a nipple even to be found (more than a minute brown spot), on the closest inspection.

Soon after commencing the use of the *ungt. hydroid.* the solution also was administered in the proportion of 36 grains to the ounce of *aq. distill.* Her stomach bore thirty drops easily, but beyond this she could not advance without nausea; the lancinating pains and occasional bleedings of the breast entirely ceased; the evening dose of opium was dismissed. After pursuing this plan for four months, all the inferior portion of the ulcer was completely cicatrized. This process taking place from below upwards, in the course of another month a patch of lint the size of a sixpence would cover the uncicatrized part. Above this, at the top, there remained a hard ridge, not half an inch long, resembling the edge of a child's little finger, quite dry, painless, and of a bright red colour. I directed this to be rubbed with the *ungt. hydroid.* twice daily, the solution to be continued; and so sanguine were my hopes now become that my poor patient would ultimately attain a perfect cure! (CURE OF CANCER!) that I cannot find language to express my feelings; but these feelings will be well comprehended by all who are enthusiasts in their profession, by all who love their species, and by all who are daily witness of the sufferings of human nature under all the varied forms of cancer.

I now saw Mrs S. only once a week, and expected to find the remaining hard edge of the old ulcer becoming softer, and yielding to the fate of all its predecessors, viz. to soften, to ulcerate, to slough away, and, like them, leave a smooth shining surface adherent to the costæ; but this was anticipated happiness destined to share the common fate. The little stubborn ridge did not yield to the rubbing with the ointment, now increased threefold in strength, but enlarged; became of a purple hue, divided into tubercles, contaminated a gland or two in the axilla; fretted edges, incurvation, gleety discharge and reopening of the fixed cicatrix soon followed, so that in two little months the size and general appearance of the ulcer had resumed its original greatest dimensions, the emaciation recommenced its career, the countenance betrayed the existence of cancerous diathesis,—and in short, nothing now can be expected but that termination to dreadful suffering which so many thousands have experienced. This narration, so calculated to exercise the reader's patience, is given with a view to exhibit the effects of iodine in a case hopeless, when it was first employed. What it is capable of effecting when called into service in the earliest stages of foul, malignant, or cancerous ulcer, remains to be proved by successive trials. That it is an agent of great power and managable qualities, I think cannot be doubted.

Chester, Oct. 15, 1825.

VI.

A Case of Irregular Origin and Course of the Right Subclavian Artery and Right Inferior Laryngeal Nerve; with Remarks.

By John Hart, Member of the Royal College of Surgeons in Ireland, Demonstrator of Anatomy at the Medico-Chirurgical School, Park Street, Dublin, &c. &c.

Early in the present winter, an old female subject was brought into the dissecting rooms of the New School, Park Street, in which I discovered that the right subclavian artery arose from the aorta direct. It came off from the extreme left part of the arch, after the origin of the left subclavian, at the left side of the body of the second dorsal vertebra. It then crossed the spine obliquely, behind the thoracic duct and œsophagus, and, coming out on the right side in front of the first dorsal vertebra, ran across the apex of the pleura, and reached the place it is usually found to occupy on the upper surface of the first rib, between the *scaleni* muscles.

The situation of the *innominata* was occupied by the right carotid, which, coming immediately from the aorta, ascended directly in front of the trachea, nearly as high as the lower border of the thyroid gland, before it took up its usual lateral position in the neck, being thus in danger of the edge of the knife in tracheotomy.

The *nervus vagus* of the right side, in entering the thorax, crossed the subclavian artery without giving off the recurrent nerve in the usual manner. The office of this nerve, however, was performed by several branches arising from the inner side of the trunk of the *nervus vagus*, the highest of which, being the largest, separated from the trunk opposite the cricoid cartilage, crossed inwards behind the carotid artery imbedded in its sheath, and, getting under the edge of the inferior constrictor of the pharynx, became, by its distribution, the inferior laryngeal nerve. The branches below this were much smaller, and supplied the thyroid gland, trachea, and œsophagus. These also crossed behind the carotid; but the branches which go to join the cardiac nerves were given off together at the lower part of the neck, and got in front of the arch of the aorta, in their course to the cardiac plexus.

The recurrent nerve of the left side passed through the arch of the aorta in the usual way.

When such an irregularity in the origin and course of the right subclavian artery exists, the situation of the vessel behind the œsophagus is supposed to constrict this tube so as to prevent the food passing freely through it, and thereby produce that species of difficult deglutition, or dysphagia termed *lusoria.** It has moreover happened that a sharp fragment of bone, accidentally swallowed and stopped by such constriction, has pierced the coats of the œsophagus, wounded the artery, and thus caused a fatal hemorrhage.†

Had it been thought advisable, during the lifetime of this particular individual, to pass a ligature round the right subclavian artery internal to the *scaleni,* increased difficulty and considerable embarrassment must undoubtedly have been experienced by an operator unprepared to meet with the occurrence, when he found that the vessel he sought, instead of coming from the front of the trachea, came from behind the œsophagus, and consequently that it occupied a deeper situation in the neck, and had more of its cylinder surrounded by the pleura.

With respect to the above described unusually short course of the right inferior laryngeal nerve, accompanying the irregularity of the right subclavian artery, I believe there is but one solitary case precisely similar to be found on record, and that is related by Dr Stedman in the Edinburgh Medical and Surgical Journal for October 1823, illustrated by a plate.

The irregularity of the right subclavian has, however, been repeatedly remarked by anatomists; but it does not appear that, in such cases, there had been any examination made of the *par vagum.*

In the earlier periods of the existence of the fœtus, the rudiment of the head appears as a small projection from the upper and anterior part of the trunk, the neck not being yet developed. The larynx at this time is placed behind the ascending portion of the arch of the aorta; while the brain, as it then exists, is situated so low, as to rest on the thymus gland, and front of that vessel. Hence it is, that the inferior laryngeal nerves pass back to the larynx, separated by the ascending aorta, the left going through its arch, while the right goes below the *arteria innominata.*

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* *Vide* Burns on Irregularities of the Larger Arteries, and the Surgical Anatomy of Mr Colles.

† Case by Mr Kirby, in Dublin Hospital Reports, Vol. 2.

As gestation advances, the head becomes more distinct, and the neck begins to be formed after the second month, which, as it lengthens, has the effect of removing the brain upwards to a greater distance, and of drawing out the larynx from the chest, in accommodation to which, the nerves of the *par vagum* and their recurrents become elongated, and hence the circuitous route the latter are found to take afterwards, forming loops in which the aorta and right subclavian artery are, as it were, suspended.

Somewhat analogous to this elongation, is that which the spermatic vessels undergo, which, having been derived from the source most convenient to the original situation of the testicle below the kidney, are drawn out and lengthened in proportion as this organ descends through the abdominal rings to occupy its place in the scrotum.

Had not the great bloodvessels been originally thus interposed between the brain and larynx, the inferior laryngeal nerves would not have been entangled by them, and we should find them in the adult taking the nearest route to their destination. Such in fact was found to be the case, on the right side, in the present instance, as well as in the case of Dr Stedman, in both of which the right subclavian artery had lain behind the trachea and œsophagus; and I will venture to assert, that every case in which this vessel is so circumstanced, will present an example of the same kind of accompanying deviation from its ordinary course, in the inferior laryngeal nerve of the same side.

A knowledge of this occasional variety should teach the surgeon to beware in exposing the right carotid for the purpose of tying it, above the crossing of the omo-hyoid muscle, lest, in drawing *nervus vagus* outwards with the internal jugular vein, as usually directed, and separating the artery from the back part of its sheath, the inferior laryngeal nerve, having taken the irregular course above described, should happen to be cut across.

Such an occurrence, we are led to infer, from the experiments of M. Magendie, would affect the functions of the larynx to a serious extent.

Stephen's Green, Dec. 7th 1825.

VII.

Observations on a Train of Symptoms which occur in many Old People, are the Precursors of Death, and arise apparently

from an Ossific tendency in the Arterial System. By HENRY VERNON, M. D., Bury, Lancashire, Member of the Royal Physical Society of Edinburgh, and Member of the Royal College of Surgeons, London.

THOUGH the following observations consist only in a few conjectural and speculative remarks on a kind of death, if the expression may be allowed, frequent in old people, they may perhaps not be altogether useless, if, by exciting attention to the subject, they be the indirect means of gaining some precise notions as to the cause of the symptoms presently to be detailed. These, for want of such precise notions, the author has frequently had occasion to hear styled, in answer to questions put by the friends, a breaking up of the constitution, or by some other such vague term. Although, so far as regards the ultimate good to be obtained from whatever mode of practice be pursued, little advantage is to be expected, yet, from the confused notion of primary cerebral excitement which the author has seen obtain with many, in consequence, amongst other symptoms, of the marked disturbance of the mental functions, blood-letting, both general and local, along with vesication by cantharides, has been had recourse to, which both *a priori* reasoning and after experience have fully convinced him to be highly injudicious, and only to hasten on the fatal termination.

Having made these prefaratory remarks, I shall proceed to describe, with as much distinctness as I can, the symptoms which form the subject of this communication, the most characteristic of which is derangement in the mental functions. They are a class of symptoms not easy of description, but yet, when presented to actual observation, will readily afford a prognostic to the eye of the sagacious physician.

The subjects of these symptoms are persons from sixty years of age and upwards. They have generally enjoyed uninterrupted good health, have had a robust active form, their complexions are florid, and they have had remarkably irritable tempers, with great inequality of spirits; their minds have also had a peculiar proneness to anticipate evil, and to exaggerate the consequences of expected difficulties.

This last mentioned morbid condition of the imagination, which through life has been just sufficient to imbue the mind with an apprehensive cast of character, at the period to which these remarks refer, assumes a more fixed and decided disposition, and amounts to actual delusion; the insane idea becomes rooted, and engrosses every thought; religion is not unfrequently the subject of delusion. The patient, at this period, displays great perverseness of temper, and disputes the propriety and expediency of every measure that is proposed for his relief, always ending the discussion with a stubborn refusal to comply.

Under this mental agony and distress, there is manifested a great propensity to seek a termination to his sufferings in the commission of suicide; and if strict watch be not kept over him, he will seize upon any means in his power to accomplish his purpose.

For a length of time previous to this change in the constitution and mental vigour, his nights have been sleepless, or passed in unpleasant and frightful dreams, from which he has awoke with a *palpitating heart*, and in a state of great distress, and with fearful anticipations. When the symptoms have arrived at the period at which I am endeavouring to describe them, sleep is entirely banished; and if there be ever any semblance of it, it is in the patient's lying moaning with his eyes closed. In this wreck of mind, the bodily health seems at first to participate little; the tongue is clean and moist; the secretions into the stomach appear healthy; the stools are natural; there is no thirst or febrile excitement; nor does he complain of headach. On applying the hand over the region of the heart, great turbulence of action is immediately detected, but it exerts no corresponding influence over the pulse at the wrist, which ranges from 75 to 90, but extremely hard and unyielding, without any vibration, or imparting a sensation of elasticity; the artery feels like a hard tube raised up against the finger, by some mechanical force applied beneath it. The patient takes scarcely any food; but whether this proceeds from actual loss of appetite, or only enters into that perverseness of temper before mentioned, it is difficult to say; be that as it may, the effects of exhaustion and inanition soon became apparent; the lips and tongue are black and arid, the stools are offensive, and in two cases there was gangrene of the lower extremities.

During the course of the symptoms, the patients have attacks resembling syncope, which generally occur whilst starting out of bed in a fit of mental agony, as is often the case.

The period taken up from the first setting in of the symptoms, to the fatal termination, is usually from one month to six weeks or two months.

Having had no opportunity of making any *post mortem* examination of these cases, the following remarks, as to the probable cause, are conjectural.

Notwithstanding the morbid sensibility of mind, and indeed actual delusion, the state of the pulse, and the absence of febrile excitement denote no undue action in the brain; but rather, in conjunction with the bodily symptoms, lead to the idea of a deficiency of stimulus of the brain, constituting a want of vigour to resist the morbid impressions of the imagination; and this state seems to result either from a too languid circulation, or from its being supplied with carbonized blood unfit to enable it to perform its healthy functions, in consequence of the gradual asphyxia, if I may use the term, arising from the impeded passage of the blood caused by the ossific disposition of the arteries, and the valvular apparatus of the heart.

If these views be correct, the impropriety of allaying fancied excitement of the brain, by blood-letting and other debilitating means, is obvious, as tending only to act, along with the cause of the symptoms, in hastening on death; and though the symptoms from the first are fatal ones, and this inquiry is strictly into a mode of death, yet it is for the interest of science, and in a moral point of view, desirable to endeavour to understand the operations of Nature even in her last mortal struggles.

In conclusion, there may perhaps a speculative question suggest itself out of this inquiry, viz. whether some species of insanity, and melancholia, where the patient is prone to end his miseries by suicide, may not depend solely upon some affection of the heart.

Bury, Lancashire, 13*th December*, 1825.

VIII.

Case of Hereditary Hæmorrhæa. By THEODORE DAVIS, Surgeon, Nailsea, near Bristol.*

ON the 24th March 1824, I was requested to see a child æt. four. He was naturally of a full habit, with fair florid complexion. Upon my arrival I found him in a completely exsanguious state—his face was of an ashy paleness, his lips blanched, his eyes glassy, the pulse at the wrist barely discernible, the motion of the heart being exceedingly languid, and he appeared very uneasy, respiring with labour, and frequently moaning; in fact, dissolution took place shortly after I had seen him. The history of the case was as follows:—A week or two previously, he had received from a fall a slight superficial cut upon the back of the head. This inconsiderable wound bled profusely for three or four days, and then began to heal. A few days before I was sent for, it had nearly healed, when, from no apparent cause, it began to bleed again. This hemorrhage continued for a day or two, when a violent epistaxis supervened, and the blood from these sources was poured forth without intermission, until the vessels seemed almost drained of their contents, and the discharged fluid was at last like water merely tinged of a scarlet hue; the hemorrhage had just ceased when I arrived, and the little sufferer was evidently *in articulo mortis.*

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* If we have room, we shall insert an abridged account of another very remarkable instance of this affection, from Hufeland's Journal.—EDITOR.

The parents of the child related the following circumstances as connected with the peculiar symptoms of the foregoing case. None of the *males* in the family of the mother had, during some generations, reached to man's estate, and they had all of them been subject to profuse hemorrhages from the most trifling wounds, (her own brother had died of a hemorrhage, succeeding to the extraction of a tooth). The *females*, she informed me, enjoyed good health, and were entirely free from the tendency to bleed excessively from superficial wounds; but if they married, their *sons* inherited from them this family curse, whilst their *daughters* were equally free with themselves from it. The mother herself had the appearances denoting a strumous diathesis, but was otherwise healthy. She has several daughters who appear strong and healthy. Of her sons, (the one whose case I have described, and another aged twelve or thirteen), she gave me the following particulars:— They had been subject to hemorrhages from slight accidents, as before stated; but they were, moreover, subject to occasional spontaneous discharges of blood, the approaches of which were always made known by symptoms of plethora. Their countenances became round and florid; their animal spirits were unusually buoyant, and they were full of life and activity. These premonitory signs had always been followed by a profuse hemorrhage, generally from the nose, unless a trifling cut or scratch gave another outlet to the accumulated fluid, which continued to escape until the vessels were drained, and the poor sufferer was reduced to extremity. Various means had been tried to remedy this evil, until at last the parents abandoned all hope of averting the progress of the malady; and they had been accustomed for some time past to leave the case to Nature. When the system seemed exhausted by the discharge, the hemorrhage usually ceased; and, in the course of a little time, the vital fluid was renewed, though evidently at the expense of much constitutional power.

Since I took the foregoing notes, I have been informed by the parents, that the son, still living, has had continued attacks of the hemorrhagic complaint, and his health appears much undermined. His joints are swollen, and subject to severe rheumatic pains, and his limbs are anasarcous.

Nailsea, near Bristol, Dec. 24, 1825.

IX.

Case of Wound in the Lungs. By JAMES MITCHELL, Surgeon, Royal Navy, Wooler. Communicated by Dr ABERCROMBIE.

IN the action off Plattsburg, in America, a marine was severely wounded in the chest by a cannister shot. From his being in a small vessel detached from the fleet, I had not an opportunity of seeing him till the day after the action, when I found him on an island under a tent, and resting on a straw pallet, in a sitting posture, with his head bending forwards, mouth open, and gasping for breath, putting one much in mind of a severe fit of asthma. His face was swelled, and highly flushed; his pulse full and bounding. Previous to examining his wounds, I bled him to the extent of two pounds, which gave great relief, and enabled him composedly to describe his sufferings; for previous to that he was unable to speak. I then examined his wound, and found a cannister shot had passed through the right breast below the nipple. On examining his back, I observed the half of a cannister shot projecting partly through a wound it had made under the right scapula. I extracted it, when a large quantity of frothy blood issued out. I held a lighted candle to the wound, but it was blown out instantly. I cleansed the wound, dressed it simply, and laid him on his back, to give vent to the effused fluids. He discharged after this a large quantity of frothy blood and mucus from the lungs, by the mouth. I repeated the large bleeding in an hour with great effect, gave him cool subacid drinks, and a dose of the sulphate of magnesia, enjoining at the same time the most rigid abstinence. During the day I was under the necessity of bleeding him seven times, amounting in all to eight pounds of blood, in order to relieve the bleeding of the lungs, and the immense irritation existing in the chest, together with the severe constitutional fever. I also gave him the following draught 4 times a day. ℞ *T. digital. gtt. x. Acid. sulph. dilut. gtt. xv. Liq. plumb. acetat. gtt. iij. Aq. font. ℥i. M. ft. haust.*

By these means the constitutional and local irritation had subsided greatly ere night, in so much so, that he could remain in the recumbent posture with tolerable ease to himself, and his breathing was comparatively tranquil. His bowels were freely opened by the salts. I gave him the following anodyne. ℞ *T. op. gtt. xxx. Vin. antimon. ʒi. Spt. æth. nitros. ʒi. Aq. ℥i. M. ft. haust.*

He spent a comparatively easy night; but towards the morning he had an exacerbation of the fever, hæmoptysis, and difficulty of breathing. *V. S. ad* ℔i. This was also followed by a speedy departure of these alarming symptoms. I continued the same plan of treatment as yesterday. During the day he had two severe attacks, though they were gradually diminishing in violence; each time I took away a pound of blood from him, and always with the greatest relief. He had now lost eleven pounds of blood from the arm, independent of what came away by the wounds, and his mouth; and from his being of a very plethoric habit, his strength was not so much reduced as might have been expected. I gave him an anodyne at night.

On the third day he was much relieved; continued the same plan of treatment; allowed him a little sago. He required no further bleeding, but continued to improve in strength daily; and by keeping his bowels gently open by neutral salts, by restraining the circulation within due bounds by the sedative medicines formerly mentioned; but above all, by causing him to live very low, almost approaching to starvation, he was completely cured in five weeks, both externally and internally, with the exception of feeling a slight difficulty of breathing on walking quickly, or up an ascent. A medical friend of mine saw him two years after this, and he was continuing well.

X.

Case of Pneumothorax. By GEORGE ALEXANDER, M. D. Communicated by Dr Abercromby.

THOMAS STEVENSON, æt. about twenty-two, was admitted into the Penang General Hospital, from H. M. S. Liffey on the 26th July 1824, apparently in the last stage of pulmonary consumption. The preceding history of the case led to the conclusion, that it was originally complicated with hepatic disorder. He had an acute pain in the right side of the chest, principally under the short ribs; he had a deep sonorous cough, with copious purulent expectoration; he was subject to profuse sweats, his bowels much relaxed, and the secretions exceedingly morbid; pulse small and quick; his strength so much reduced that he could not get up from his cot without assistance. The means adopted in the first instance, were those calculated to lessen the derangement of the digestive organs; blue pill and gentle aperients. At the same time blisters were applied to the seat of pain, and expectorants with digitalis administered. Under this treatment he remained stationary for some time; and about the beginning of September he began to improve, his bowels became more regular, and the secretions more healthy; the cough was less troublesome, and the expectoration diminished in quantity, and lost its purulent qualities. He gained strength; and to the great surprise of his attendants, began to walk about. He was now considered convalescent, and the only medicines administered were with the view to keep his bowels in order, and to relieve a slight cough which remained. In this state he remained during the months of September, October, and November, there being no ship to discharge him into.

Early in December, all the unfavourable symptoms with which he was admitted returned; pain in the chest very acute, but situated higher up than before, principally under the fifth and sixth ribs. The expectoration increased, and became decidedly purulent; his bowels got into the same disordered state, his tongue acquired a florid, red, glossy appearance. Similar remedies to those first resorted to were adopted, but without material relief. The symptoms increased, colliquative sweats and diarrhœa came on, he gradually lost strength, and on the 28th December he died.

Examination of the Body four hours after death.—On opening the cavity of the abdomen, the most remarkable appearance that presented itself was the situation of the liver, which was protruded by the diaphragm from the right to the left side, so that the right lobe lay directly over the spine. The stomach was consequently displaced, and was found lying longitudinally on the left side of the *vertebræ*. The diaphragm on the right side presented a convex projection into the cavity of the abdomen, occasioned by distention from air, and thereby caused the displacement of the liver. The sternum was then carefully removed, leaving the pleura; and when this membrane was cut into, the whole right side of the chest presented a hollow cavity, with about eight ounces of thin pus at the bottom, but without the least remnant of lungs.* The heart was small, and the coronary veins turgid. The lungs on the left side were partially diseased, and interspersed with tubercular lumps.

It may be necessary to add, that the liver, though larger than natural, and of a dark livid colour, did not show any appearance of its having been the seat of active inflammation.

It may lead to some physiological conclusion to notice, that the spleen was unusually small, weighing only ʒxj. and gr. xvj.

Prince of Wales Island, 15*th January* 1825.

XI.

Further Remarks on Tetanus. By William Briggs, M. D.

(*Continued from p.* 87 *of the preceding Number*).

It is somewhat remarkable, that in the same Number of this Journal, in which Mr Manifold's case of tetanus, cured by purgatives, appears, there should also be an account of one successfully treated by *different* means, (large and repeated bleedings,) by Dr Alexander of Prince of Wales' Island. How long the disease itself had existed, when the patient was found, is uncertain, (for he could give no account of himself, only that he had been *left* by a French vessel, " exposed to the sun, without food or covering, and fell into the state in which he was admitted;" but on the first day of *treatment,* 40 oz. of blood were taken *at one time,* and 24 leeches were applied to the abdomen; on the 2d day 30 oz.; on the 4th day, 28 oz.; on the 5th, 28 oz. (126 oz. in five days); and again, 32 oz. on the 12th day.

Nothing is more commonly objected, as one of the *opprobria medicinæ,* than that the same disease has been reported to be cured by opposite modes of treatment; and to no disease perhaps is the remark more applicable than to tetanus. Histories have been published from time to time, of cures effected by means apparently the most opposite; bleedings, bark and wine, purgatives, opium, hot bath, cold bath, turpentine, tobacco, mercury, &c. &c.; which very diversity is one of the strongest proofs of the imperfection of our knowledge of the disease itself. Some may go farther, and say no other proof is necessary to establish the correctness of Dr Parry's inference, that all such cases must have been of that description, which "would have recovered with *any*, or perhaps *without* any remedies."

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* In all probability the lung was only very much compressed, and closely bound down to the mediastinum, so as to escape observation, a case which has occurred to ourselves.—*Editor.*

It does not necessarily follow, however, that because a train of symptoms has been said to be broken by *different* means, it would therefore have given way, had *nothing* been done; and, for my own part, I am induced to give credit to a large proportion of them all, as so many real cures. But then, it can only be on some *very broad* common principle, that evidence seemingly so contradictory is to be reconciled; such as that, whatever may have been the plan adopted, it *has been pushed to a great extent*, so as either to produce, on the system, an impression strong enough to *interrupt the morbid action*, but not so strong as to *extinguish life* in vigorous subjects, or else to give increased energy to the vital powers, so as to enable them to resist the shock. But, after all, it is allowed that tetanus, for the most part, proves fatal; and that what has answered in one case, is apt to disappoint when again tried. The grand defect is, that no sufficient data are afforded to ascertain the proportion of *failures* and *cures* under each mode of treatment; yet this is precisely what is the most wanted to guide the perplexed inquirer in the time of need.

As to bleeding—I would in the first place suggest, that some caution is requisite in applying to diseases, as they are seen in this country, rules drawn from India, as to either *identity* or *treatment*; because, *there*, they are in general much more strongly marked, and run their course with such frightful rapidity, that the *quickest* means of arresting their progress, must, *at all hazards*, have the preference; as in *cholera*, for instance. On this head, the evidence of Dr Burrel of the 65th European regiment, published in the "Reports of the Epidemic, under the authority of the Bombay Government," is very striking. He speaks of the "*spasms* having been, in many cases, so violent as to require *six men* to hold the patient. In such cases, venesection was carried *ad deliquium*; the patients being, at the same time, in the hot bath at 110; the spasms were invariably relieved; nausea and vomiting alleviated, so that the stomach bore calomel in scruple doses, combined with laudanum, which doses were frequently repeated." Dr Burrel had 100 admissions in three weeks, and sums up as follows—"Bled 88—dead 2. Not bled 12—dead 8." To the same effect is the testimony of almost all who have given the result of their experience in India. Mr Annesley in particular, whose work is reviewed in the last Number of this Journal, speaks of the "violent spasms affecting the whole muscles of the body, *generally of a tetanic nature*, and some so purely such, as to lead a careless observer *to confound the disease with tetanus.*" The reviewer adds, "On the one hand, a vast body of unconnected practitioners agree, that when a *large quantity* could be drawn, success was almost certain; and, in a very few rare instances only, was it followed by sudden death; which many, viewing the outward symptoms only, might very naturally be led to dread." (From this, however, it seems to be admitted, that in some very rare instances, sudden death *did* follow; which very circumstance should inspire practitioners with some degree of caution as to the *grounds* on which they direct bleeding *ad deliquium.*) "On the other hand, the abstraction of a *small* quantity seems to be of little use. Sometimes the *return of the signs of collapse* renders a repetition of the blood-letting necessary."

Fortunately, in this country, we have not much idea of such a disease as this. The majority of those affected with what *we* call cholera, do pretty well without such active practice; and, even of those who fall victims, the proportion for whom a bleeding *ad deliquium* would have been thought advisable, is very inconsiderable indeed; though perhaps almost every experienced practitioner may have occasionally witnessed a *mixed* case, where, by the timely use of the lancet, under apparently doubtful circumstances, the patient has been rescued from a very critical situation; and possibly, if the season for bleeding were not so frequently past, when medical advice is first sought, it might, even here, be found the surest way of cutting short the disease.

But to return to Dr Alexander's case, and taking it as it stands, I should say, that for *such symptoms in any climate*, I should not have scrupled to order venesection; but then I should have thought that I was prescribing for a *febrile inflammatory* affection, with tetanic symptoms superadded, rather than for *simple tetanus.* The pulse was, from the first, "100, full and hard; blood buffy and cupped; tongue foul and dry; urine highly coloured and scanty;" and all these remained till 126 oz. of blood had been taken. (N. B. It does not appear that deliquium was ever produced in this case; perhaps had it been so, the cure might have been fully as speedy, and there might have been a saving in the whole quantity taken). Again, we perceive none of that insensibility of the system to *even very large doses,* whether of *opium* or of *purgatives,* which so generally, but not *universally,* accompanies tetanus, and compared with which, the quantity of either, directed by Dr Alexander, would seem to be but as dust in the balance; yet it does not appear that there was any want of them; the bowels were, for the first four or five days, kept sufficiently open by clysters, and two common draughts. How far the bleedings tended to *preserve* the natural sensibility of the bowels, just as, in cholera, Dr Burrel found its most immediate effect was to *settle the stomach*, is an interesting question, of which more below.

But I was led, by Dr Alexander's narrative, to turn to two cases, of which I happened to have brief notices, though I had forgotten the particulars, and which seem to confirm the propriety of the *depleting practice, in certain cases*, perhaps even more decidedly than his own; because both of them were *traumatic*, and occurred in northern climates in *December;* the one, in the Military Hospital at Chelsea, published in the 11th Vol. of the Medico-Chirurgical Transactions, by Mr Burmester; the other at Quebec, published in the London Medical and Physical Journal for August 1819, by Dr Painchaud. In both of these, however, there was, as in Dr Alexander's case, considerable *febrile excitement*, particularly in the former; so that for a week it was regarded as "simple continued fever;" but, in one day, complete tetanus was formed—with "lacerated tongue, skin hot and dry, pulse full and strong, insatiable thirst."—"With a view of *diminishing vascular action*, and allaying irritability, 36 oz. of blood were immediately taken; he was immersed in a warm bath; an aperient draught was administered, and pilul. hydrarg. gr. v. with opii gr. ss. directed to be given every 2 hours; ʒii. of ung. hydrarg. fort. to be well rubbed into the thighs and external fauces, night and morning; and a draught of æther and tinct. opii ā ʒi. at bedtime. Next day, considerable relief, but *pulse quick and full;* blood-letting repeated, (quantity not specified); and, in the evening, his pulse, notwithstanding the quantity of blood taken in the morning, being considerably *increased* in strength and fulness, 16 oz. *more were taken*." On the following day (the 3d), "the pulse, after having been about 90, soft and regular, had again increased in *strength and frequency*, and 28 oz. more of blood were taken." In this case, therefore, there were at least 100 oz. of blood taken in the first three days. I would solicit a comparison of it, with that of the boy selected by Dr Parry, to illustrate his doctrine of the inevitably fatal course of *acute* tetanus. Here the most active part of the treatment *had all taken place*, by the time that, in Dr P.'s case, the pulse had risen to 96. (Again I would observe, that *deliquium* is not named). Mr Burmester adds, "had not active remedies been *timely* resorted to, the patient would in all probability have died; and it would be a matter of some difficulty to decide, whether the copious depletion, the mercury, or the warm-bathing and diaphoretics, were individually the most effectual in promoting his recovery; for I am inclined to believe, that neither of these, *singly persevered in*, would have produced the same favourable result." Perhaps so; but with such recollections as mine, it is natural for me to believe, that all these were *not* alike effectual, and that if with the *copious depletion*, drastics had been substituted for the mercury, diaphoretics, &c., the same *favourable result* might have ensued, and in fully as short a time.

Altogether, however, the case is very important; and the more so, as Mr Burmester's is precisely the sort of evidence which ought to have the greatest weight. He was with our army in the Peninsula, and speaks of "the *very numerous* cases of tetanus which fell under his observation." Yet even this evidence is defective in the main point; he does not mention the proportion of *failures*; he does not even say that he had so much as *tried* the same treatment before; neither does it appear that the bleeding was ever followed by *deliquium*; but he mentions another fact, which is, in my opinion, still more instructive: A soldier had tetanus, from a musket shot, between the metacarpal bones; mercury, the warm bath and opium, had been largely used, without effect, and the patient appeared to be dying. When *gangrene* came on, followed by considerable sloughing, "the locked jaw subsided, and he ultimately recovered, with the loss of a considerable portion of the fleshy part of his hand." Mr B. states farther, that he "very generally found *warm bathing* and the use of diaphoretics alleviate the symptoms; whereas the *cold bath*, as far as his experience goes, has only aggravated the spasmodic convulsions, and produced the most frightful contortions of the body." But again, he gives no particulars as to *temperature*, &c., though it is natural to conceive, that in army practice, there would be considerable variety in these respects; and we should not lose sight of the fact, that Dr Currie's patient, Gardner, a soldier, was *cured* very quickly by the cold bath, of idiopathic tetanus, (occasioned by circumstances not unlike those to which Mr Nisbett's patient had been exposed, as detailed in the last Number of this Journal), after being reduced apparently to the last extremity. Before Dr C. saw him, "he had been bled and vomited repeatedly, and had used the *warm bath*, not only without alleviation, but with aggravation of his complaints." (Here again no particulars are stated). General convulsions came on; opium was pushed till he could no longer swallow the pills, when laudanum was substituted, till in the last 26 hours, he took ℥vss.; "the debility was extreme, a complete hemiplegia had supervened, the patient's eyes were fixed," &c. After a suspension of all medicine for six days, he seemed to revive, but a *convulsion* then came on as severe as ever, " followed by a return of *all his symptoms with their former violence.*"...He was *thrown headlong* into the salt water bath, at the temperature of 36. *The effects were instantaneous.*' As he rose from the first plunge, he stretched out his left leg, which had been for some time retracted to the ham;—he was plunged down and raised to the surface successively, for upwards of a minute longer, the muscles of the neck relaxing more and more after every plunge. " When taken out, we felt some alarm; a *general* tremor was the only indication of life, the pulse and respiration being nearly, if not entirely, suspended." But by warmth, friction, &c. the vital heat returned; the patient slept comfortably upwards of two hours; and when he awoke, to the astonishment of every one, *he got up and walked across the room, complaining of nothing but hunger and debility.*...He continued the bath daily for about a fortnight, and in less than a month was under arms again." Dr C. adds, that " his experience of the cold bath, in tetanus arising from *wounds*, is neither so satisfactory, nor so complete." There is, however, in the Number of this Journal for January 1808, a striking case of *traumatic* tetanus, cured by cold *affusion* by Mr Arnoldi of Montreal. " Upwards of 26 buckets of water were poured on the patient's head, when we " *perceived his pulse to flag*, and he sunk down in his chair and the jaw dropped. *We were apprehensive that we had carried it too far*, but *in the course of few minutes*, the reaction commenced by the help of warmth, *wine*, &c.; he continued the plan for 12 or 14 days, and gradually returned to health." Mr Arnoldi adds, " I am strongly inclined to think that the benefit to be expected from the cold affusion will be very little, *unless it is carried to the length of producing a suspension of the vital principle.*"

These testimonies are, I apprehend, no inapt illustration of the effect of *bleeding ad deliquium*, as practised by Dr Painchaud, whose case is still nore conclusive than Mr Burmester's, in as much as the propriety of bleeding was hardly so clear. It was *traumatic* tetanus; the symptoms had come on suddenly; and the first medical attendant, finding " the pulse quick and hard," had *bled* the patient, (to what quantity is not stated), but he " observed that it *increased* the spasms." (Here one cannot but remark, how exactly the experience of practitioners in tetanus, during a *Canada winter*, corresponds with that of those in India, for cholera, as to the futility of *small* bleedings). So severe were the symptoms, that, at a consultation of *three* medical gentlemen, *before* Dr Painchaud was applied to, it was concluded, " that the only thing remaining to be tried was *amputation above the knee.*" When Dr P. first saw her, " the whole body was bent like a bow, and supported only on the back of the head, and on the heels; the jaws were so closed that it was impossible to introduce the blade of a knife; and he thought her on the very point of expiring.—"Yet," says he, " the *success I had already met with from copious bleedings*, in similar cases, (*this* is, after all, the grand test; though even here there is no account of the *failures*), prevailed on me to try the same measures; and finding that her pulse, *although small and rapid*, much resembling such a one as commonly attends inflammation of the brain, held out tolerably well, I immediately came to the resolution of bleeding her *till she fainted.* I was obliged to take 36 oz. The fainting fit lasted a long time, but the contraction of the jaws, and the general spasm, yielded visibly to this powerful depletion. ... After two hours she relapsed, and as violently as before. I repeated the bleeding, which was followed by a fainting, after the fresh loss of 18 oz. During three days she took, each day ℥iss. of laudanum. On the fourth day her mouth again closed, and the *same convulsions* began:—Another bleeding *ad deliquium* (30 oz.), and the patient found herself relieved, *as if by enchantment.*"..." I gave her gr. 60 of calomel, and 60 of opium, each day, during three successive days; and during six others, gr. 60 of opium *alone*, each day. The calomel did not cause any salivation; it acted powerfully on the bowels, from which it brought away several worms. The woman is at present perfectly well."

Here is another case, which it is difficult to bring within Dr Parry's rules; the pulse was *small and rapid*, when Dr Painchaud first saw the patient, which could not have been later than the fifth or sixth day, (the critical period with Dr Parry). In fact, nothing seems wanting but the *fatal termination*, to entitle it to a high rank among his *acute* cases.

From the effect of *deliquium* produced by *bleeding*, or the *cold bath*, one is naturally led to the consideration of another powerful sedative, *tobacco*, which has been recommended by Dr O'Beirne in the 3d volume of Dublin Hospital Reports; and on this subject I am in possession of a narrative which, if I were quite sure as to the facts, would be most important; and considering how well it corresponds with the reported effects of *deliquium* from other causes (though I have had it by me for more than a dozen years), also that my informant (the surgeon of the vessel, who was almost an entire stranger to me, and whom I have never seen or heard of since), *took no credit to himself*, I would willingly hope his account may be correct. The case is said to have occurred on board of a Greenland ship, in latitude 70, and to have been traumatic, and after much had been done, it was given up as hopeless. But while the surgeon was at dinner, the sailors tried some rough practice of their own; they boiled ℥i. or ℥ij. of tobacco in a pint of stale urine, and forced as much of this abominable compound down the poor fellow's throat as they could; so that, when the surgeon returned, he found him apparently dying; but violent vomiting and purging came on, and the patient *was able to do his duty as a sailor within the week.* When the danger was over, the truth came out, but not before; for so long as the sailors thought they had killed the man, they kept their own counsel.

It was not till after the former part of these remarks was actually sent to the press, that I turned to the two cases published by Mr Burmester and Dr Painchaud; but I do not scruple to acknowledge, that *my own views* are still farther modified; and particularly by the fact mentioned by the former, that in a case, apparently desperate, the symptoms gave way on the coming on of *gangrene.* * Not that I have met with any thing to shake my confidence in *drastics*, or the theory of *counter-irritation*, on which I was originally induced to give them; quite the contrary; neither am I prepared to give up my former conviction, that, " in tetanus, the *danger to life* is by no means in proportion to the intensity of the outward affection;" or that, where the symptoms most to be feared is a disturbance of the functions of the *heart*, it can always be advisable to induce *deliquium*, whether by bleeding, tobacco, or cold immersion, or affusion.

With respect to the cold bath, more particularly, even in the successful cases recorded by Dr Currie and Mr Arnoldi, they were themselves afraid they *had gone too far*, and seemingly not without reason; the impression made on the system was indeed strong enough to *break the disease*, and *that* very quickly and completely; but had it been only a *very little stronger*, or if the rallying powers had been *less strong*, it would have *extinguished life.* In short, from all the evidence, it would appear as if the cold bath were the least eligible of all the three modes of producing a strong impression on the system. By universal consent, however, *moderate* measures, whether in the way of sedative or the contrary, are seldom of much avail in tetanus, and serve only to throw discredit on the remedy. Wherever, therefore, sedatives of all kinds are not manifestly contra-indicated, I should think it a capital improvement in practice, *always to begin with one* or more copious bleedings; and I am greatly mistaken, if a much smaller quantity, both of bleeding and of drastics, when *combined*, would not be found to answer, than when either of them is trusted to without the other. Perhaps *wine* would come in better, if preceded by depletion.

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* This fact appears to furnish an additional reason for applying caustic freely to the wounds in tetanus. It may be remembered that, in my remarks subjoined to Gaskell's case, I mention how speedily an anomalous spasmodic affection, not unlike hydrophobia, for which a variety of remedies had been tried without effect, yielded on the application of a caustic to an old contused wound, which was completely cicatrized at the time.

The cases where there could be the *least* difficulty, would, of course, be those of the description adduced by Dr Alexander and Mr Burmester, where there was considerable "febrile excitement, pulse full and quick," &c. But Dr O'Beirne, who was stationed with the artillery in the Peninsular war, where "he saw about 200 cases," and who had the *sole charge* of the wounded officers and men of the artillery at Brussels, after the battle of Waterloo, states, that he *never saw a case of tetanus attended with fever.* In another part of his excellent paper, in the Dublin Hospital Reports, he makes the farther appalling statement, that he *did not see a single instance of recovery;* nor did he hear of one, though at the latter station "he was in constant communication with the medical officers of ten different hospitals."—"But to speak with candour (says he), my impression is, that the antispasmodic plan was more attended to than the purgative." *

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* Dr O'Beirne says, he "never saw a case of tetanus arising from a wound of the *head,*" which is certainly a very curious fact. But, in reply, I am authorized by my friend Charles Wingfield, Esq., one of the surgeons to the Radcliffe Infirmary, Oxford, to state that, last July, he had a fatal case, from "a contused, lacerated, very extensive *wound on the scalp,* forming a complete flap, and exposing the pericranium to a very considerable extent, of which, however, the cranium was not in any degree denuded." It was occasioned by the falling in of a bed of gravel on a man that was employed in making a deep drain; and the wound was completely filled with dirt and gravel, which could not be wholly removed. Yet so well did he appear to be doing, in every respect, that on the tenth day he was to have been made an *out-patient;* but while his wound was being dressed, preparatory to his leaving the house, he spoke to Mr W., for the first time, of a *stiffness about his jaws,* which, on farther inquiry, had been coming on gradually for three days, though still he felt himself so well, in all other respects, that he was with difficulty persuaded to remain a few days longer; yet he died on the evening of the sixth day from this time, or at the end of *nine* days from the first feeling of the symptoms. (Here then is another exception to one of Dr Parry's rules).

I am also authorized by my friend Mr Forshaw, one of the surgeons to *our* Infirmary, to state, that the last case of tetanus he had was from a *contused wound on the forehead,* in a private patient whose horse had fallen with him. The wound was cicatrized before the symptoms came on, when Mr F. was first consulted; but it manifestly contained gravel, and he opened it. This patient recovered; but as the gentleman who attended him may perhaps publish the case, I am not at liberty to

One would almost conclude, however, that there ought to be a *separate chapter for Tetanus, as it occurs after battles;* when, from the combined effect of, perhaps, much previous exhaustion, bodily and mental, succeeded by the greatest excitement and exertions, and these again by the depressing passions, and probably also by cold, hunger, and hæmorrhage, there must, of necessity, have been a very large expenditure of the *vis medicatrix;* insomuch that, for *this whole order*, one would think that, as a general rule, *bleeding* was inadmissible, and that *cordials, with the warm bath and drastics*, should be immediately resorted to; always remembering, however, that Mr Burmester's patient, in whom severe tetanus gave way, on the coming on of *gangrene*, was also serving in the Peninsula; and that the complaint was occasioned by a gunshot wound in the hand. But this was comparatively a *slight* wound, and probably would be reported as such; and, at any rate, the man would *not be left exposed on the field of battle.*

Very different are the circumstances under which tetanus usually occurs in this country, especially at such a place as Liverpool, where the great bulk of those who are the most liable to be affected by it, are persons engaged in active employments *out of doors*—a class of people in whom, of all others, the *rallying powers* are, on the whole, in the greatest perfection; and among *such*, I apprehend the utility of copious bleeding would be pretty generally manifest. I am quite ready to admit that in *some*, or perhaps *all* of my own *successful* cases, whether *minor* or *major*, and especially in that of *Wright* (who could not bear wine), bleeding, even *ad deliquium*, might have been beneficial. And if such a case as that which I saw for Dr Jeffreys, and which ran its course so rapidly, were to occur to me again, I certainly should have immediate recourse to bleeding, more particularly as so little else could be done; at any rate, the event could not have been *worse*. Many a time, too, has it occurred to me, as a speculation, respecting my female patient in the Workhouse, especially since I have heard of the practice in India, whether, if I had paid as little regard to the *coldness, weak fluttering pulse*, &c., as the medical men *there* pay to the

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say more than that the treatment consisted chiefly of brisk purging and strong home-brewed ale. It is probable, however, that Dr O'Beirne's remark is generally true; for Mr Wingfield "has heard from other surgeons *that they never knew tetanus from a wound on the head.*" I confess myself alike unable to account for the general rule, and for the exceptions, otherwise than by observing, that wounds on the head (*in battle*) will probably have been inflicted by a *cutting* instrument, such as a sabre (musket-balls will generally have proved fatal); whereas, in the cases which occurred to Mr Wingfield and Mr Forshaw, the wounds were *contused*, with gravel impacted in them.

same symptoms in *cholera*, and, instead of *wine* and *drastics*, had relied on a bleeding, followed by calomel and opium, to restore the balance in the system, she might not have done quite as well: better she could not have done; but where the most that can be said for a powerful two-edged weapon, is, that *perhaps* it might have answered *as well* as a less formidable one actually *has* answered; *who* would employ the one, so long as the other was to be had?

As to Mr Nesbitt's patient, considering how nearly the circumstances under which he was originally affected, corresponded with those which preceded the attack of Dr Currie's patient, Gardner, perhaps if bleeding or the cold immersion had been *early* resorted to, and pushed *ad deliquium*, or if he had got tobacco clysters, the result might have been different; but from the time of *my seeing* him, probably there are not many who would have thought that his was a case for *sedatives* of any kind. On the contrary, I must always regret that more *wine* was not given to him, or perhaps brandy. Still more decidedly should I say of such a case as that quoted from Stoll, that I cannot conceive a period when any prudent practitioner would have thought *that* patient a fit subject for venesection. Just as soon should I think of bleeding indiscriminately in *fever* as in tetanus.

On the other hand, as to *wine*: Highly as I appreciate it, in combination with other remedies, I am at a loss to imagine how it can ever, of *itself*, have cured tetanus, otherwise than by enabling a *citadel naturally strong to hold out till the assailing force was spent*; and it appears to me that the case reported by Dr Currie admits of this explanation. It has been stated, that this patient sucked in a quart of port wine in two hours, "when he thought himself refreshed. At the end of twenty-four hours he had finished his third bottle; and at this time it was evident that the down-hill progress of the disease was checked. *Though the pain under the ensiform cartilage was little abated, yet he found himself more able to bear it.*" But indeed it was not to wine *alone*, even in this case, that the cure was intrusted. "Every night he took from 60 to 120 drops of tinct. opii, and, *with the wine*, this small dose seemed to have a more soothing effect than thrice the quantity before he began it." There is one circumstance noted by Dr C., which is perhaps of more importance than he was aware of. "On the 15th day of this course, he was affected with *slight nausea and vomiting; and thenceforwards his recovery seemed more apparent.*" After all, as I have elsewhere observed, one cannot feel much surprise "that it was not till the patient had been forty-two days under this treatment, and had swallowed 110 bottles of wine, that his safety could be ascertained; or that, at the end of ten years, he should retain the indelible impressions of the disease." Yet, as "opium and mercury had been largely administered, with first the cold bath, and then the hot bath, and the disease proceeded with the most unfavourable omens, till the wine was given," one cannot but ascribe the recovery to it; just as when it was the practice, almost indiscriminately, to drench *fever patients* with wine, without attending sufficiently to the state of the head, bowels, &c. *Here and there one*, who would otherwise have sunk, may have been enabled to struggle through. Nevertheless, I cannot but think that, if this man had been *early* bled *ad deliquium*, or if *less wine* had been given, and powerful drastics along with it, *or if all the three* things had been done, the recovery might have been both quicker and more complete. Indeed my wonder is rather that such practice should ever have succeeded at all; and I have the authority of my friend Henry Park, Esq. to say, that though he was one of the Infirmary surgeons at the time, and attended this patient along with Dr Currie, he has never once known it succeed since. A much more consolatory statement, is one which Mr Forshaw authorizes me to make; namely, that he has been connected with this Infirmary as pupil, house-surgeon, and surgeon, upwards of thirty-five years, with little intermission, and *that tetanus does not occur half so frequently as it did within his own memory.*

Again, I cannot comprehend on what principle mercury, of *itself*, can ever be expected to perform a cure in tetanus. But, in fact, neither is *it* ever trusted to alone; and of the *combination* of calomel with opium, I can only say, that it has long and deservedly been a favourite formula with me, in *many* complaints; and, so far from having any prejudice against it in tetanus, my friends can testify that there were moments in the treatment of *Wright's* case, when I wavered as to the propriety of persevering with drastics, to the exclusion of opium, in *traumatic* tetanus, and was inclined to substitute this combination for them. I now rejoice that I did not; for, I would repeat, it *could not have answered much better;* and here is an important practical question settled, namely, that the practice is as applicable to the one species as to the other.

But however I might have been disposed to waver *then*, the case is very different *now*. I remember some curious calculations in one of Dr Haygarth's works on small-pox, as to the very great difference in the weight of testimony, if *one*, if *two*, if *three* escape infection in given circumstances. On this principle I would take my stand, and say that, with such cases as those of Gaskell, Wright, and Dr Carson's patient, with two or three others, not indeed so strongly marked, but decidedly of the same family, *all on one side*, and with the solitary exception of Mr Nesbitt's patient on the other, and, more particularly, when that exception may be so explained as not materially to affect the character of the remedy, a body of evidence is brought together, which cannot be overturned but by one of greater weight.

Liverpool, February 10*th*, 1826.

XII.

Case of Fractured Femur with Inversion of the Toes. By JAMES SYME Esq. Lecturer on Surgery. (*With an Engraving.*)

IN the last number of the London Medico-Chirurgical Transactions, there is a very interesting paper by Mr Guthrie, on Fracture of the Neck of the Femur; the object of which is to prove, that inversion of the limb occasionally results from the accident in question, and to explain how this effect is produced.

Owing to the weight of the limb itself, and the predominance of those muscles which tend to perform rotation outwards, namely, all that connect the pelvis and femur, excepting the glutaeus medius and minimus, together with the tensor vaginæ femoris, eversion of the toes is so regular a symptom of fractured thigh-bone, as to make most surgeons consider it an essential one. Boyer says, " we are not acquainted with a sufficient number of instances in which the foot turned inwards, to enable us to admit that this even happens occasionally. *We have not seen it*, and it is difficult to conceive how it can take place, time, further observation, and, above all, anatomical inspection, can alone furnish the solution of this problem. " Mr Sam. Cooper, in the last edition of his Dictionary, that laborious, faithful, and most valuable record of surgical facts and opinions, thus expresses himself in reference to the occurrence of inversion. " Such an appearance of the toes is very uncommon, and the reason of it in the case alluded to has not been explained. "

Mr Guthrie having met with a case of fracture at the upper part of the femur, in which the limb, after being for some time everted as usual became remarkably inverted, had his attention particularly directed to this puzzling symptom, and at last met

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Fig. 1.

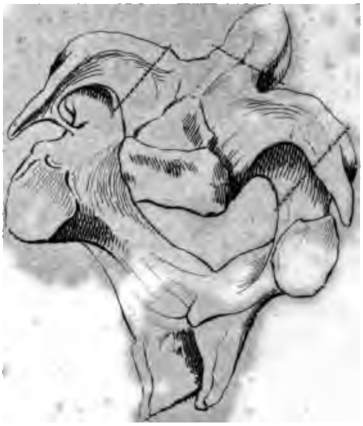


Fig. 2.

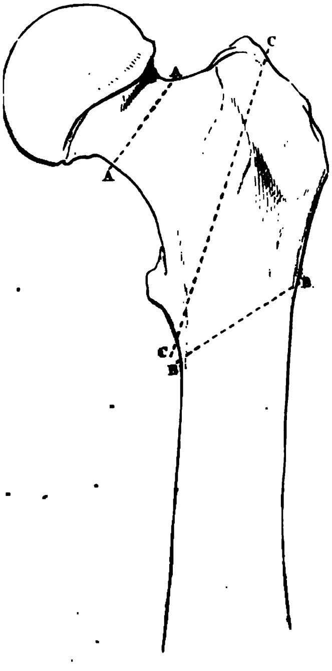
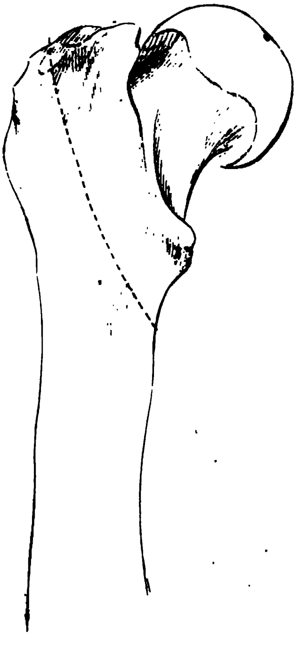


Fig. 3.



with an opportunity of examining the state of parts concerned in its production.

"Sarah Gibson, aged 90, fell, on the 9th of January, from a high stool on which she was sitting, upon the left hip, and being a heavy woman, suffered considerable injury. I saw her two days afterwards, with Mr Dillon of Judd Street, and found the marks of a considerable contusion having been sustained by the part, which was very painful and swelled. The limb was rather more than half an inch shorter than the other, and the great toe turned inwards, in a manner sufficiently marked, although not quite so decidedly as in a case of dislocation.

The limb was moveable in every direction, but these motions were attended by considerable pain, and it could be scarcely extended to the same length as the other. The patient died on the 22d of February, forty-four days after the accident, and, on dissection, a fracture was discovered external to the capsular ligament. The little trochanter was broken off, and with it the attachment of the psoas and iliacus muscles. The head and neck of the femur were separated from the shaft by a diagonal fracture, extending from the upper and outer part of the trochanter major to the trochanter minor, so as to leave the insertions of the pyriformis, gemellus, obturator externus and internus, and quadratus, with the head and neck of the bone. The glutaeus medius formed a bond of union at the upper part of the trochanter major, between the broken pieces, retaining them in contact. The capsular ligament was not injured, and no steps whatever appeared to have been commenced to repair the mischief which had been committed."

From this case, and a careful consideration of the various powers which affect rotation of the thigh, Mr Guthrie concludes, that the toes will be turned inwards wherever the fracture is so situated as to separate along with the head that part of the trochanter major to which the muscles rotating outwards are attached, and leave connected with the shaft that part of the same process which receives the insertion of the glutaeus medius.

To make the matter plainer, I may use this simple diagram. See Fig. II.

When the bone is fractured in the direction AA. the weight of the limb, together with the six rotating muscles inserted behind the trochanter major, and the flexors attached to the trochanter minor, will overcome the effect of the glutaeus medius, &c. and turn the toes outwards. When the fracture is in the direction BB., the weight of the limb, uncontrouled by the rotators will cause it to be turned outwards; but when the fracture is in the direction of CC, then the obturatores gemelli, pyriformis, and quadratus femoris, inserted behind at the root of the trochanter major, together with the iliacus and psoas attached to the trochanter minor, exerting all their effect on the detached head and neck, while the anterior part of the glutæus medius remains connected with the shaft of the bone, the toes will be turned inwards.

Some time ago, in the course of my dispensary practice, I was asked by one of the pupils to visit a patient who had met with a severe injury of the hip. I found a tall, stoutly framed man, apparently between 50 and 60 years of age, lying on a sofa, complaining loudly of his left hip. The limb was shortened more than an inch, the knee being bent over the sound one, and the great toe resting on the instep of the other foot. The trochanter major was higher up and more forward than natural, and any movement excited excessive pain.

The patient told me, that, two days before, he fell from the elevated pavement upon his side, and immediately felt unable to rise, or even move, being previously paralytic of both arms. The passers by, thinking him drunk, afforded no assistance, until at last, one more charitable and discriminating than the rest, raised him up, procured a coach, and sent him home.

I at first naturally enough supposed, that there was a dislocation upwards and backwards upon the dorsum of the ilium; but finding that moderate extension restored the limb to its due length, and feeling, while this was doing, a pretty distinct crepitus, I recognised the existence of fracture, and lost no time in applying the long splint of Dessault.

I next day mentioned the case to my class, as being one in which there was much reason to believe that a fracture had occurred in the direction CC.

The patient felt relieved as far as the limb was concerned; but died at the end of two weeks, in consequence of inflammation in the lungs, and most extensive sloughing on the sacrum.

On dissection, I found the capsular ligament entire, but two fractures beyond; one detaching the neck when beginning to spring from the shaft, the other running obliquely downwards and inwards through the trochanters, so as to detach the posterior part of the greater, and the whole of the lesser, in one piece from the body of the bone. See Fig. III. The preparation is in my Museum.

This case tends, I think, very strongly to confirm the important diagnosis of Mr Guthrie, and I have therefore much pleasure in making it public.

6, *Forth Street,*
15*th February* 1826.

XIII.

Case of Recovery after extensive Exfoliation of the Vertebra Dentata. By JAMES SYME, Esq., Lecturer on Surgery. (*With an Engraving.*)

IN the beginning of August last, Mr G., aged about 35, applied to me on account of difficulty in swallowing, which had troubled him for nearly twelve months; at first very slightly, but more and more progressively, until at last it had become exceedingly distressing.

Finding that he was unable to swallow large morsels, or indeed any thing but fluids—that he felt equal difficulty in vomiting as in deglutition, and that he was constantly spitting what seemed to be merely saliva; observing also that he had that peculiarly anxious expression of countenance which attends stricture of the œsophagus, I concluded that the dysphagia depended not upon relaxation of the parts, as the patient supposed, but rather upon some mechanical obstruction. I attempted, without success, to pass a bougie; and then introducing my finger, discovered a large tumour filling up the lower part of the pharynx—pressing forwards upon the epiglottis, and leaving merely a narrow channel on each side. It had a smooth surface—was of firm consistence—and apparently proceeded from the posterior part of the pharynx, where it was immoveably attached.

In consultation with two gentlemen who had previously been acquainted with the case, I proposed to puncture the tumour, which we were all agreed in suspecting to contain a fluid; but this was objected to by one of the consultants, on the ground, that as openings of abscesses are most beneficial when dependent, we, being unable to cut into the tumour except at its upper part, ought to let Nature take her own way; and our practice, for some time, consisted in occasionally passing a small steel bougie through the lateral canal, left by the swelling, in order to relieve the patient's suffering, and promote a natural discharge by ulceration.

In the course of a fortnight the patient's sufferings became excessive—the swallowing of fluids even being now effected with extreme difficulty. But this was not the worst of his complaints. For a good while past he had frequently awakened from sleep in great anxiety for breath, owing to the obstruction of respiration caused by the tumour; and now dreaded beyond expression the approach of night, when he was sure to suffer a terrible struggle between exhaustion and the horror of immediate suffocation. To give some idea of his dreadful state at this time, I may mention, that the physician in attendance frequently assured me he would not be at all surprised to find him no more at our next meeting.

Mr G. at last peremptorily insisted upon something being tried for his relief, when, with approbation of all concerned, I introduced a long curved trocar, and withdrew an ounce or two of thick yellow pus; immediately upon which, he assured us, that, he was perfectly free from all the uneasy feelings which had so long distressed him; and we found, in fact, that there was hardly any trace of the tumour left. On examining the cavity of the abscess with a probe, I was concerned to find the bone deprived of its coverings; but this we flattered ourselves was an effect, not the cause, of the disease.

It is impossible to describe the joy and satisfaction of our patient on being thus suddenly relieved. Suffice it to say, that some of his friends, who called soon afterwards to condole as usual, were received in a manner so different from that to which they had been accustomed, as to convince them that extremity of suffering had deprived the poor man of his senses.

Next day there was some slight return of swelling, the wound having closed. I therefore introduced a long blunt-pointed curved bistoury, and forced it into the opening, which I then enlarged freely downwards. After this the patient suffered in no respect, excepting, as had been anticipated, from food entering the abscess. In a week this inconvenience was no longer felt; and he departed for the country, with a view to the complete restoration of his health.

In the end of August Mr G. returned to his usual employment in town, and remained well until the beginning of October, when he began to complain of severe shooting pains through the neck and occiput, together with some slight impediment to deglutition. On examination I could discover no fluctuation, but merely a general fulness in the posterior part of the pharynx.

The pain, though at first somewhat alleviated by common remedies, continued to increase, and towards the end of October became so severe, and so cruelly exacerbated by motion of the head, however slight, as to require confinement not only to the house, but to one posture—any alteration of which was matter of serious reflection, and tolerable only when executed with such circumspection as to prevent the joints of the head and cervical vertebræ from being at all affected.

About this time I made a free incision through the puffy swelling of the pharynx, and found extensive disease of the bones. The patient's strength and appetite now diminished rapidly; the discharge from his throat was profuse, and insufferably fetid, in short, the melancholy scene seemed fast drawing to a fatal conclusion.

In December, another physician who was consulted proposed an attempt to remove the bone, but this I declined, feeling persuaded that the disease comprehended its entire thickness; in which case, any effectual operation must have proved very dangerous, or rather certainly destructive, to the spinal marrow and vertebral arteries.

On the 12th of January I was sent for to remove the bone, which was said to be loose; and, to my surprise, actually found a large portion projecting into the pharynx through the old opening. I made some attempts at extraction; but feeling that the detached bone was of very considerable extent, and fearing that my efforts might cause it to injure the spinal marrow, particularly as the patient nearly fainted from the gentle means already tried, I contented myself with freely enlarging the wound in both directions, and left Nature to finish the extraordinary work she had so far advanced. Two days afterwards, the bone escaped so far into the throat, as to be taken out with the patient's own fingers.

The pain now ceased entirely; there was no longer any fetid smell from the throat; and no complaint remained but weakness. It is hardly necessary to state, that the horizontal posture was strictly enjoined; indeed the patient's own feeling sufficiently indicated the propriety of this. At the end of a fortnight he sat up for a few minutes, and has now the prospect of being speedily restored to perfect health, without any shortening or other visible alteration of the neck.

Fig. I. represents a front view of the vertebra dentata. The dotted line shows the extent of bone actually detached in one piece, and now in my possession, which includes all the body, part of the left superior articulating surface, and half of the left vertebral hole; but, if we allow for the absorption of living bone necessary to permit the separation of so large a dead piece, the destruction of vertebra must have been much more extensive than here delineated. Indeed, for a week or two previous to the great separation, many small fragments of bone were almost daily coming away.

6, *Forth Street,*
15*th February,* 1826.

XIV.

Case of Introsusception. By JOHN VALENTINE, Member of the Royal College of Surgeons, and Licentiate of the Society of Apothecaries, of London.

THE publication of extraordinary cases, when they occur, is the duty of every medical practitioner. Although, from one case alone, deductions of any great importance cannot be made, yet, from the accumulation of well verified facts, much good may eventually accrue. In drawing up the following case of introsusception, I shall merely give a statement of facts; only observing, that it is one amongst many instances which show how great things Nature can accomplish, and that, under the most unpromising circumstances, we may sometimes hope for a favourable result. *

CASE.—On Wednesday, October 22d, 1823, I was requested to visit, at 9 *p. m.*, JOHN CULLY, æt. 40, a poor labourer, and found him affected with a violent pain of the bowels, increased on pressure, bitter taste in the mouth, sickness, occasional vomiting, and some fever. He has had no evacuation since yesterday, and describes the pain as if girt round the body with a rope. I took away twenty ounces of blood, and ordered a common enema to be administered, and to be repeated if necessary. Pills, composed of scammony, jalap and calomel, to be taken every four hours, and his diet to consist of diluent beverages, such as gruel, tea, &c.

23d.—From a multiplicity of professional avocations, I was prevented from seeing him till late in the evening, when I was informed, that he had had no evacuation, the abdomen being much distended, accompanied with an urgent desire to void urine, without the power of doing so. I introduced the catheter, and about half a pint of urine was drawn off, which gave great relief.

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* The cures which are frequently affected by Nature alone, occasionally surpass all belief, unless they were well authenticated. Conversing with a friend a short time since, on the case which forms the subject of the present paper, and on whose veracity I can place the utmost reliance, he related an instance which he himself saw at Bushire, in Persia. A horse belonging to Lieutenant Charles Paisley, of the E. I. E. military service, and secretary to Lieutenant-Colonel Malcolm, on his first embassy to Persia, was afflicted with so great an introsusception, that the intestine reached the ground, in such a quantity as to fill a common-sized pail. It separated, the animal recovered, and was afterwards used in the campaigns of India.

He is to take eight grains of calomel immediately, and a wine glass full of infusion of senna, with sulphate of magnesia, every four hours, until the bowels are freely opened. He is also to be placed in the warm bath for half an hour.

24*th*.—*Mane*. Has had several evacuations, but still in occasional pain.

From this period to the 5th of November, he suffered under a constant diarrhœa, which was only partially relieved by aperients, absorbents, astringents, and large doses of *tinct. opii.* On that day, I was sent for in great haste to see him, the relations being much alarmed by his passing a large portion of intestine, which, on my arrival, I found to be the case. It appeared from its size, and the colour of the feces adhering to its internal coat, to be a portion of the colon, measuring twenty-eight inches, which is now in my possession. From the 24th of October to the period of his passing the portion of intestine he had been subject to frequent pains of the abdomen, and the evacuations in the course of a day and night were generally from ten to fourteen in number. After passing the intestine, he was in some measure free from pain, but still had an intensely bitter taste in the mouth. He was directed to observe the mildest diet, and to take at bed-time the following pill:

℞. *Opii gr. ij. Pil. hydrarg. gr. iij. Ft. pilul.*

November 6th.—*Mane*. Pulse 92; has had a good night; no pain; experiences some tenderness of the abdomen in the left iliac region when pressed; has had two loose evacuations of a yellow colour; no sickness; disagreeable taste abated; perspired freely in the night.

℞. *Mist. salin. fixt.* ℔ss. *Sumat coch. iij. quartis horis.* Diet to consist of milk, gruel, broths, &c.

Vespere.—Countenance pale; tolerably free from pain; pulse 98, full and strong; no fever; gentle perspiration; has had several evacuations from the bowels of a darker colour; bitter taste still continues, although the tongue is clean; urine high coloured. Has taken the mixture regularly.

Rep. pil. opii cum hydrarg. hora somni.

7th.—*Mane*. Passed a very good night; bitter taste still continues; pulse 95, full and strong; has had no evacuation during the night, till this morning, which is very loose, copious, and of a dark colour; urine high coloured; pain on pressure in the left iliac region; no sickness or vomiting. *Contin. mistur.*

Vespere.—Pulse 95; general perspiration; bitter taste still continues; tongue clean; has had two evacuations of a yellow colour since my visit in the morning; diet still to consist of diluent beverages.

Contin. mist. salin. et repetet. pil. anod. hor. som.

8th.—*Mane.* Tolerable night; three evacuations, not very copious; pulse 95, full and strong; general perspiration; tongue rather furred; bitter taste still continues; urine high coloured.

Contin. mist. salin.

Vespere.—In frequent pain; pulse 78; several evacuations; great anxiety depicted in the countenance, and tenderness of the abdomen.

Repet. pilul. opii et hydrarg. h. s.

9th.—*Mane.* Pulse 78; has had an easy night; several evacuations, copious, of a light colour; pain diminished; troubled with hiccup; a slight fur on the sides of the tongue, and bitter taste; urine deposits a copious lateritious sediment; thirst.

Vespere.—Pulse 86; hiccups; has had from ten to fifteen evacuations since my morning visit, of a light colour, and slimy; flatulency; pain and tenderness of the abdomen.

Repet. pilul. anod. hora somni.

10th.—*Mane.* Passed a good night; tongue rather furred; pulse much the same; urine high coloured.

Vespere.—In considerable pain, which he compares to griping; tongue furred; bitter taste still remains; urine high coloured; pulse 78, soft, and occasionally intermitting; hiccup not so frequent; has had several evacuations.

Repetat. pilul. anod. hor. somni.

Sumat cras primo mane ol. ricin. ℥*ss.*

11th.—Pulse 78; has had a good night; tongue rather furred; tenderness and pain of the abdomen abated; bowels moved three times; urine deposits but little sediment; took the castor oil this morning, as ordered yesterday. Diet to consist of gruel, broths, &c. as usual.

Vespere.—Pulse 80, full, and strong; the oil has operated several times, producing copious stools of a dark colour; hiccup nearly left him; tenderness much diminished; appetite good. Diet to consist of mutton or beef broth, milk, tea, &c.

Repetat. Pil. anod. h. s.

12th.—*Mane.* Pulse 78; passed a comfortable night; tongue furred; thirst; urine rather high coloured.

Vespere.—Pulse 90, regular, full, and soft; has had three evacuations of a dark colour; has frequent griping pains.

Repet. Pil. anod. h. s.

13th.—*Mane.* Pulse 78; no evacuation since last visit; urine high-coloured.

Vespere.—Pulse 84; three evacuations since the morning; appetite good; sat up to-day for a quarter of an hour.

Repetat. pilul. anod. h. s.

14th.—*Mane.* Pulse 78; tolerable night; has had three evacuations; free from pain; tongue clean. He is allowed, in addition to his diet of milk, broths, &c. rice, and a little roasted mutton.

Vespere.—Pulse 72; no evacuation since my visit in the morning; free from pain; perspired greatly in the course of the day.

Repet. pilul. anod. sine pil. hydrarg. h. s.

15th.—*Mane.* Pulse 78; passed a very good night; tongue little furred; has had no evacuation, and feels very little tenderness of the abdomen.

Vespere.—Pulse 76, full and strong; has had three evacuations since the morning visit; tongue clean; feels a pain in the left side, about the eighth and ninth ribs, which increases a little on inspiration. He has sat up to-day for half an hour.

Repetat. pil. anod. hora somni.

16th.—*Vespere.* Pulse 78; tongue clean; has had four evacuations to-day; appetite good; pain in the side nearly gone.

Repet. pil. anod. h. s.

17th.—*Mane.* Pulse 84, full and strong; tongue quite clean; good appetite.

Repetat. pil. anod. h. s.

18th.—*Mane.* Has had a bad night, scarcely any sleep; great flatulency of the stomach; frequent and griping pains of the abdomen, and tenderness in the left iliac region; urine clear; has had four evacuations since last night of a light colour; tongue slightly furred; pulse 82, soft and regular; very little thirst; the evacuations were not offensive.

Sumat. gran. j. opii statim, et gr. ij. h. s.

19th.—*Mane.* Feels easy; tolerable night; one motion only since last visit; pulse 88; tongue clean; urine rather high-coloured; has a good appetite, and no thirst.

Repet. pil. anod. h. s.

I conceive it to be wholly unnecessary to detail the symptoms and treatment of this case any further. Suffice it to say, that up to the first of January, he was generally obliged to take two, sometimes four grains of opium every night, which was absolutely necessary to restrain the laxity of the bowels and occasional griping pains. He is now perfectly recovered, but has commonly two evacuations daily.

Mr Langstaff, in an excellent paper on this subject, published in the third volume of this Journal, has the following passage. " In many of the cases which are published, and in that which is related in the present paper, the seat of the disease was clearly denoted by a hard tumour in the left side of the abdomen. This circumstance, together with the impossibility of throwing up more than a very small quantity of fluid by the way of glyster, which was strongly marked in the present instance, and the presence of the other symptoms, would lead us to suspect the nature of the disorder." To the observation made in this quotation, my patient furnishes an exception; no less than a quart of fluid was thrown up and retained. In Mr Langstaff's case it was of course impossible, as the intestine protruded to some distance from the rectum, as it was supposed, in consequence of the frequent efforts to void the contents of the bowels. *

In concluding this paper, the following observations on Introsusception by M. Andral, in his Memoir on the Morbid Anatomy of the Digestive Tube, may not be deemed irrelevant, but interesting to every surgical practitioner. "May not the irregular and the multiplied contractions, of which the muscular membrane of the inflamed intestine becomes the seat, be regarded as one of the causes of *invaginations of the intestines?* Boyer has well described the mechanism of these invaginations, from observations on living frogs. By irritating the intestines of these animals, he saw them contract forcibly in several parts, whilst in others they remained distended with aliment and flatus; these latter portions he saw receive the former. "Hæ ita ampliatæ intra se receperunt constrictas intestini portiones, easque sinu suo absconditas aliquamdiu detinuerunt, donec fibris se denuò exporrigentibus intestini pars una e latibulo alterius, velut e domunculo limax, pristinam in sedem rediret."

But although several authors have remarked that they have constantly found the mucous coat very much inflamed at the part where *intus-susceptions* existed, and have regarded them either as the cause or the effect of such inflammation, we can affirm, that they more frequently exist without inflammation of the intestines. † It is probable that many of them are formed only at the moment of death, a period at which the peristaltic motion of the intestines becomes much more active, as experiments made on living animals demonstrate.

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* Edin. Med. and Surg. Journal, Vol. III. p. 264.

† Inflammation is very far from being a necessary attendant; it is even uncommon. See Burns' Midwifery, p. 585.

Dr Baillie states, "that in opening bodies, particularly of infants, an intus-susceptio is not unfrequently found, which had been attended with no mischief. The parts appear perfectly free from inflammation, and they would probably have been easily disentangled by their natural peristaltic motion. At other times, however, so large a portion of the gut passes within another portion of it, that it cannot be disentangled, and the passage of the intestines is obstructed, without any possibility of its being set free. This is the fatal state of the disease, is sometimes attended with inflammation, and is sometimes without it."—Morbid Anatomy, pp. 172, 173.

Considered with relation to their seat, intestinal intus-susceptions are far from being equally frequent in the different portions of the digestive tube. The ileum is the most common situation in which they occur. Fabricius Hildanus, and Bartholin have seen the cæcum received into the ileum; Hartmann, on the contrary, found the ileum received into the cæcum. *Invaginations of the colon are very rare.* Meckel says he has seen the transverse and descending colon introduced into its sigmoid flexure. Bonetus has related a case of invagination of the rectum in a man who died of obstinate obstruction of the bowels, accompanied with vomiting of a fæcal matter. Can we believe the assertion of a German writer, who says he has seen the duodenum invaginated in the ductus choledochus?

With regard to their disposition, the superior portion of the intestine is most commonly received into the inferior. Sometimes invaginations are very numerous; we have observed as many as seven in the same subject. Their length may vary from some inches to upwards of two feet."—See Medico-Chirurgical Review, Vol. 4, pp. 678, 679.

Burns, in his Principles of Midwifery, says, "that in one case, no fewer than 47 intro-susceptions were found in the same body. Invagination of the intestine is, in children, the most frequent cause of fatal diarrhœa; not less than 50 cases occurred to his brother, in the course of his dissections."—Burns' Midwifery, 4th edit. p. 584.

Somerton, Someretshire, Feb. 18, 1824.

XV.

Case of Amaurosis from Metastasis, terminating favourably. By THOMAS LESSEY, Member of the Royal College of Surgeons, London.

THE singularity of the following case may perhaps render it sufficiently interesting to obtain its insertion in your Journal, although I am not aware that any new or important practical conclusion can be deduced from it; unless it may be deemed of importance, as a precedent, to hold out some hope of recovery, in similar cases, by the mysterious agency of the *vis medicatrix naturæ.* I have endeavoured to sum up briefly the important points of the case, avoiding tedious and unnecessary minutiæ of detail; in doing which, I hope I have not fallen into the opposite error of being too vague and indefinite in my description.

ELIZABETH HEALEY, a slender delicate young woman, about five-and-twenty, of a sedentary occupation—pale countenance, emaciated figure, and feeble melancholic temperament, applied to me on the 9th of June 1820, for relief of an affection of the bowels, to which she had been liable for several years, requiring, even in a comparative state of convalescence, the constant use of purgatives, to prevent the occurrence of an obstinate constipation, which followed their slightest neglect. Indeed the general derangement of the abdominal viscera was so great and so permanent, as to induce the belief that it was of an organic nature. In addition to this, she was liable to frequent and severe cephalalgia, and occasionally to attacks of dyspnœa, accompanied with spasms of the chest and throat, which, on attempting even to swallow liquids, produced alarming symptoms of suffocation. These attacks came on suddenly, and with great violence; they were accompanied with great feebleness of the voice, which remained after the more urgent symptoms had subsided, and were succeeded by great exhaustion and debility.

She had tried various remedies with temporary success. Her bowels had been frequently relieved by mercurial and saline cathartics; the attacks of cephalalgia by venesection, and the application of leeches and blisters to the head and neck; and the more formidable affection of the lungs by various remedies which had been employed.

On her first application to me, she was labouring under an attack of obstinate constipation, with great pain in the bowels, not confined to one particular part, but extending over the whole abdomen. Her voice was very weak and feeble, and her breathing short and difficult; but by no means so severe as to excite alarm. In the course of a few weeks, by the brisk use of cathartics, both mercurial and saline, the affection of the bowels was considerably relieved. The dyspnœa was more difficult to manage; however, by the repeated application of blisters to the chest, occasionally keeping them open with the Lytta ointment, I succeeded in removing it; but still the feebleness of the voice remained unaltered, and her general debility was considerable. She recovered gradually, but slowly, for two or three months, when she discontinued the use of medicines, and returned to her occupation.

She had another attack of the bowel complaint in the January of the following year 1821. Being myself indisposed, my friend Mr Barrow attended her for a fortnight; and the affection was yielding to his remedies, when she was suddenly seized, on the 23d, with a most violent attack of dyspnœa, threatening immediate suffocation. I was instantly sent for, and found her in a most distressing situation. Every attempt to swallow, or even to speak, was followed by a kind of convulsive spasm of the throat and chest, attended with frequent sobbing. A few doses of ether and opium, with a blister on the sternum, relieved the immediate urgency of the symptoms; but still the breathing continued laborious, and her voice, which had long been very feeble, was now reduced to a scarcely audible whisper. The derangement of her abdominal viscera returned; her stools were green and slimy; her pulse was feeble, and her general debility so very great, that I despaired of her recovery.

She remained in this state, with little variation, until the 15th of February, when the difficulty of breathing *suddenly* left her, and her *voice* became *distinct*, *strong, and clear!* but a sudden, and very violent pain seized her *head*, and she *screamed* out *loudly* for help, to the astonishment of the people with whom she lodged: they hastened to her assistance, and found her in an agony of pain, and quite *blind!* I immediately ordered her head to be shaved, and a blister to be applied to it, with a dozen of leeches to the temples; which abated the violence of the pain, but produced no alteration in her sight. The eyes were fixed, and nearly motionless—the pupil steady at a medium point, between contraction and dilatation, and totally insensible to the action of light, as I satisfactorily convinced myself by repeated experiments. On presenting a candle suddenly to her eyes, she exhibited no consciousness of its presence, unless it was sufficiently near for her to feel the *warmth* of its rays. I next applied blisters to her temples, dressing with the Lytta ointment, and frequently repeating them, so as to keep up a continuous discharge for several weeks:—the bowels continued torpid, and required the constant use of purgatives. I next tried the use of the blue pill, and her gums were slightly affected, but without any effect on her sight. Her voice continued strong, her breathing easy, and, in fact, the affection of the chest appeared to have left her entirely. The pain in the head was considerably abated, but the vision remained so entirely lost, that all hopes of its recovery were abandoned; and having declared my opinion that it was an incurable case of amaurosis, and as she procured her means of subsistence only by her needle, she was compelled to enter the Manchester Workhouse in despair.

Three months after her admission she had a severe attack, both in her chest and bowels—obstinate constipation—dyspnœa, with violent spasm, and great difficulty of swallowing—which lasted three weeks with violence, and subsided slowly.

At the latter end of last year, 1822, she had a slight attack of pleurisy, which yielded to bleeding, blistering, and the usual mode of treatment; since which she has been tolerably free from all complaints, excepting slight headachs.

Although she entertained little or no hope of again recovering her sight, yet she occasionally (to use her own expression) "tried her eyes with a candle;" and, on the evening of the 29th of October, she tried them with the usual effect. She perceived no glimmering whatever; but to her great surprise on the following day, towards evening, as a person was conducting her through the streets, she saw a confused appearance of fire, and exclaimed, "what is the matter with my eyes?" and, in the course of a few minutes, she discovered that it proceeded from the gas-lamps, which she saw indistinctly.

Her sight gradually improved the rest of the evening; and on the following day, Thursday, I saw her myself. There is considerable mistiness and obscurity in her vision, with *muscæ volitantes* of a fiery hue, more or less troublesome:—but still she can distinguish the features of her acquaintances, and could read the large print capitals of a handbill, which was accidentally lying on my table: the smaller print seemed confused, and blended together. All distant objects are mixed up with coloured mists, and consequently are indistinct and confused.

On the 20th of November she called on me again. Her sight remains much the same as when I saw her last; but had been better during the interval, and was injured again by injudicious exposure to a highly heated room, which she felt very oppressive to her feelings, and annoying to her sight. The coloured mists occasionally trouble her still, and sometimes *muscæ volitantes* are very numerous. To use her expression, "they are mixed with white flakes like snow, and resemble them in shape and size." I found she could not read better than when I last saw her; but, with the help of a double concave glass, she could distinguish print, which, to her naked eye, was a confused mass. Convex glasses rendered the confusion greater. Her eyes are naturally prominent; but never required the use of glasses before. She has no complaint remaining—her bowels and her lungs have been free from disease for the last twelve months, and she exults in the prospect of her recovery from a series of most overwhelming and protracted afflictions.

Manchester.

XVI.

Letter to the Editor of the Edinburgh Medical and Surgical Journal, explaining the Object of the Society of Physicians of the United Kingdom.

In consequence of having seen in your 85th Number, p. 396, the following paragraph, "We grieve to see attempts made at the present day, in the formation of a Society, to exclude any one branch of the healing art, from a participation of its public labours and usefulness," I am induced to point out to you the mistake under which the reviewer appears to me to labour, in regard to the objects and intentions of the Society of Physicians of the United Kingdom, to whom I suppose these remarks refer. This Society does not, as I apprehend, exclude any individual of the medical profession from a participation in their public labours and usefulness. On the contrary, one of their rules expressly states, "the publication of papers furnished by members of the Society, or of those which may be transmitted to them *by the profession at large.*" But the object of the society, as I understand it, is twofold; the promotion of medical science, and the protection of their own peculiar interests, so far as they are distinct from those of the other branches of the profession. In this latter intention they only follow the example of the other departments of the medical profession, each one having its own separate association, the admission into which is confined to the individual members of that department. The regularly educated physicians alone have no common centre of communication or of action; they are completely in an isolated state. It cannot be denied, that they have their separate interests distinct from the other branches, and surely there can be no objection to their associating for the protection of such interests. How can they admit into such an association persons who possess no common feeling nor common interests with themselves. They profess to confine their members to persons practising the *consulting* department of the profession only, not from any notion of other departments being less qualified, or less desirous to promote medical science, but because, as every society must have some limits of admission, they have chosen the above, as being well defined, and embracing those individuals *who have no claim to be admitted into any other distinct association.* The system of exclusion is not on their side, but against them. Will the College of Surgeons admit the regularly educated physician of Edinburgh and Glasgow into their number, and protect his interests in common with their own? Will the Company of Apothecaries do it? Will the College of Physicians of London do it? No. Are they, then, always to remain unconnected individuals, and be the only branch of the profession who are denied the liberty of associating together, on the only ground which they possess in common, regularity of education and restricted practice, viz. consultation alone, without any interference with other branches, or participation in any department in which manual assistance is employed? Were the promotion of medical science by medical men at large, their *only* object, their association would be unnecessary, since there are already several societies, in which every department of the profession is indiscriminately admitted, to some of which the members of the association in question belong; in this town, the Medical Society of London, the Medico-Chirurgical Society, the Hunterian Society, &c.

A slight view of the present state of medical practice, will suffice to show the expediency, if not the absolute necessity of the regular physicians associating for mutual protection; and the extent of the system of encroachment and of exclusion practised against their class.

1*st*, The physician-accoucheur claims, as being peculiarly qualified, the exclusive privilege of prescribing in all cases of diseases of children, and in those of pregnant and parturient women, and he considers this department of practice as appropriated to himself. But whilst he thus endeavours to exclude the general physician from interference in his own peculiar department, he does not scruple to invade the province of the other, and to practise indiscriminately in *all* cases, whatever be the age, sex, or disease of the patient.

2*d*, The pure surgeon, whilst he would loudly and indignantly complain of any attempt on the part of the physician to interfere in the department of surgery, and will scarcely suffer him even to look at a cutaneous affection, scruples not to practise in every case of internal disease whatever, and to claim the right of medical practice in an equal degree with the regularly educated physician. And it is well known, that in the practice of our most celebrated surgeons, medical cases form, by far, the greatest proportion of their business. Ought not the restriction in both of these cases, to be mutual? Is the regular physician to be rigorously excluded from participation in these branches of practice, whilst his own is to lie open to every invader?

3*d*, The apothecary, or general practitioner, rivals the regular physician in medical practice, and it is well known that the greater number of this branch of the profession use their utmost endeavour to exclude the regular physician from any interference in cases under their management. Without denying the competency of the general practitioner to be equal with that of the regular physician, (though taking the former as a body, this will scarcely be allowed), or questioning their right thus to act if they please, it may be asked, if this exclusion be practised against the latter, may they not, and indeed are they not compelled to take the *sole* management of cases when offered to them? Yet this is imputed to them as encroaching on the privileges of the general practitioner; they must suffer encroachment but not retaliate.

4th, Did no other motive exist for the regular physicians to associate, that of wishing to preserve the respectability of their own class would be sufficient. It is now the common practice with the most ignorant and illiterate pretenders in medicine, to furnish themselves with a medical diploma from one of those Scotch Universities, who, to their eternal disgrace, traffic in degrees, and bestow them, without examination, on persons the most unqualified to possess them. The public unfortunately confounds these two classes together; and a Scotch physician is become almost a term of degradation. An association which separates the regularly educated physician from the ignorant intruder, surely does not deserve the censure of the profession.

5th, The regular graduates of Edinburgh and Glasgow are, as a body, equal, in point of medical education and medical acquirements, with those of Oxford and Cambridge; yet all the privileges of the College of London are confined to the latter, who scruple not to exercise the most unjust monopoly against their excluded brethren. This system of exclusion operates greatly to the prejudice of other physicians, especially in London, it being now almost impossible to obtain an appointment to a public institution, against the influence of the College, which is invariably exerted in favour of the Fellows. Should it be said that the public at large regards not these distinctions, and that the licentiates obtain their share of employment, it may be replied, that often in questions where the public are materially concerned, they (the public) have no choice; and all the talents and acquirements of the whole body of physicians, except of the Fellows of the College, are absolutely denied to them. I refer particularly to all such questions as are submitted for professional investigation by Government or by Parliament; these are referred to the College, who never, on any occasion, admit the licentiates to a share in their deliberations. Even in the published literary Transactions of the College, no production by any licentiate or regular graduate can be admitted without having a mark of degradation prefixed to it; the paper must be *communicated* by a Fellow.

To rescue themselves on the one hand, from undeserved degradation, and unjustly usurped authority, and on the other hand to separate themselves by a broad line of distinction from ignorant and illiterate pretenders, with whom the College absurdly attempts to confound them; to submit fairly to the public their claim to public patronage and support, by evidence of their united contributions as a body, are surely objects which may be properly entertained by that class of physicians who form, and who are invited to join this association; and whilst they studiously refrain from interfering with any other department of the medical profession, and are truly anxious to uphold the respectability, and promote the interests of the whole profession at large, they may expect to be allowed to pursue their plans, without deserving obloquy from, or experiencing the censure of, other practitioners.

MEDICUS.

London, Nov. 24, 1825.

PART II.

CRITICAL ANALYSIS.

I.

*An Inquiry into the Nature and Treatment of **Diabetes**, Calculus, and other Affections of the Urinary Organs; with Remarks on the Importance of attending to the State of the Urine in Organic Diseases of the Kidney and Bladder: and some Practical Rules for Determining the Nature of the Disease from the Sensible and Chemical Properties of that Secretion.* By WILLIAM PROUT, MD. FRS. The Second Edition, Revised and much Enlarged. London, 1825.

THE first edition of this work appeared in 1820, and we must freely confess, that we failed to do justice both to the author and to our readers, by omitting to make them acquainted with its value. In the following analysis of its contents, we fear that we have not succeeded in repairing our fault; for the subjects it embraces are of such practical importance, and are treated of in such a masterly and condensed manner, that we have been obliged to pass over many observations, which we were inclined to notice; but we earnestly recommend the whole to the attentive perusal of practitioners of every denomination.

In the Introduction Dr Prout treats of the composition and general properties of the urine, makes some general remarks on urinary derangements, and explains his division of the subject of his volume. To his account of healthy and morbid urine, he very properly premises a summary description of the blood from which it is formed, and gives the following contracted tabular view of their constituents.

BLOOD contains	URINE contains,	
	Healthy,	*Diseased,*
Water.	Water.	———
Albumen, fibrin, red particles.	——— ———	Albumen. Fibrin. Red particles.
———	Urea.	———
———	Lithic acid.	———
———	———	Nitric acid. Erythric acid. Purpuric acid. Melanic acid? Oxalic acid. Benzoic acid. Carbonic acid. Also xanthic oxide. Cystic oxide. Prussian Blue? Sugar. Bile.
———	———	
———	———	
———	———	
———	———	
———	———	
———	———	
———	———	
———	———	
———	———	
Lactic acid, and its accompanying animal matters.	Lactic acid, and its accompanying animal matters.	——— ———
Sulphur. Phosphorus. Muriatic acid. Fluorine?	Sulphuric acid. Phosphoric acid. Muriatic acid.	——— ——— ———
———	Fluoric acid?	———
Potash. Soda. Lime. Magnesia. Silex?	Potash. Soda. Ammonia. Lime. Magnesia. Silex?	——— ——— ———
———	Mucus of the bladder.	———
———		———
———	———	Pus.

This Table is elucidated by valuable remarks on each of the substances which occur in urine, with a reference to the state in which they exist in blood; and, from the consideration of the changes operated upon the blood by the kidneys, Dr Prout is led to adopt the opinion advanced by Berzelius, that *acidification* is the characteristic action of the kidneys. By their healthy operation, the sulphur and phosphorus of the blood is converted into sulphuric and phosphoric acids, and lithic acid is generated from its elements. In disease, this acidifying tendency is sometimes excessive, and nitric acid, oxalic acid, &c. are produced; on the other hand, it is occasionally diminished, suspended or subverted, and neutral substances, as urea and sugar; or even alkaline substances are generated in abundance, while the sulphur and phosphorus of the blood pass unchanged. He concludes the Introduction with a few general remarks—*1st*, Functional operations are necessarily circumscribed within narrow bounds; 2*d*, A disposition to diseases of the urinary organs seems to be frequently inherited; 3*d*, Persons subject to urinary affections often suffer from different forms of these diseases at different periods of their lives; 4*th*, When acids are found in excess, the urine is commonly small in quantity, and high coloured, and the disease inflammatory; when neutral or alkaline substances are formed, the urine is generally pale-coloured, and larger in quantity, and the diseases are those of irritation and debility.

"The practical inferences to be drawn from these general remarks, which might be much extended, are most important, and should be constantly kept in mind. From them we learn the deep seated and constitutional character of urinary diseases in general; their intimate connexion with each other, and the important information respecting the nature of any particular disease, to be derived from the examination of the urine; they guard us also against the absurdity of trifling with supposed specifics; of considering the more rare forms of disease as anomalies, and, at the same time, direct us to modes of treatment founded on precise and rational principles." p. 35.

The work itself has two great divisions; one of which treats of the functional, and the other of the mechanical and organic diseases of the urinary organs. He first considers diseases in which the presence of an albuminous principle in the urine is a characteristic symptom. Our readers already know, that Dr Prout, on another occasion, has shown that albumen is not an uniform principle, but that its properties are very different, as it exists in blood and in chyle. It occurs in urine in both states, very rarely indeed serous, more frequently chylous, but much more frequently it has an intermediate character; and this observation leads us generally to remark, that a very common error prevails in regard to the chemical state of the animal principles, as if they always possessed the properties ascribed to them by writers of chemical systems, who have described them from characteristic specimens; whereas they are exceedingly modified in different animals, in the same animal at different ages, and under different circumstances, and even in different parts of the same animal; and although chemists, from extreme examples, have described mucus, gelatine, albumen and fibrine, we constantly meet with substances of an intermediate nature. An extreme case of chylous urine, which occurred to Dr Elliotson, was examined in three specimens by Dr Prout; one voided in the morning, another after breakfast, and the third in the evening. They were all chylous; but the third so much resembled chyle, that Dr Prout says, he doubts if it had been brought to him as a specimen of that fluid, that he would have discovered the imposition. It consisted of a solid coagulum of a white colour, and assuming the shape of the vessel like *blanc-mange*. We pass over an instance of a slighter form of the disease, and proceed to extract his conclusions.

"A chylous condition of the urine may occur at all ages; but those in whom I have seen the ordinary forms of the affection most frequently take place, have been past the middle age, of an irritable scrofulous habit and impaired digestive powers, and who frequently have been free livers. In such habits more particularly, and perhaps in any under certain circumstances, this condition of the urine may be excited by a variety of causes, such as a long course of mercury, stimulating diuretics, violent passions of the mind, exposure to cold, &c. Frequently, however, it will be found that this affection cannot be traced to any particular cause.

"With respect to the tendency and danger of this affection we have seen that slighter degrees of it, in which the tendency may be considered as simply *passive*, can exist for years, without apparently becoming worse, or producing any serious effects on the constitution. The danger of the affection, however, must of course increase with its permanency and degree, though it may be remarked, that even in the extraordinary case above related, which may fairly be considered as of an extreme character, the constitutional symptoms were by no means severe; and, what is still more singular, and apparently characteristic of its simply passive character, it did not even seem to interfere with the important function of generation.

"From what has been said, it will be readily seen, that in this condition of the urine we can hardly lay down any specific plan of treatment, which must, therefore, depend very much on the nature of the disease with which the affection happens to be complicated. Considered as a symptom, however, it may, in many instances, be useful in directing us to avoid certain remedies, such as stimulating diuretics, especially those of the alkaline kind, &c. the employment of which, for the most part, will be likely to do mischief in this affection. Sedatives and tonics also may be occasionally useful."

Serous urine is much more rare; and Dr Prout, though exceedingly cautious in his expressions, evidently does not concur in the doctrines of Dr Blackall in their full extent; and, from our own experience, we have been long satisfied that neither is albumen always found in inflammatory dropsies, nor is the presence of albumen a sure indication of the propriety of bloodletting.

Diseases in which an excess of urea is the characteristic symptom, are treated of in the second chapter; and they are the more interesting as the subject has hitherto escaped notice, or been misunderstood for other diseases, especially *diabetes insipidus*.

"The average specific gravity of the urine in these complaints seems to be a little above 1.020, and occasionally to vary from 1.015 to 1.030. Most generally it is pale, but occasionally it is high coloured, and exhibits somewhat the appearance of porter, more or less diluted with water; and this variety in appearance not unfrequently takes place in the urine of the same person. When first voided, it reddens litmus paper. For the most part it is entirely free from sediment, except the mucous cloud of healthy urine; and the only remarkable property which it appears to possess is that of containing abundance of urea, so that on the addition of nitric acid, crystallization speedily takes place. From the quantity of urea present, it is very prone to decomposition, and soon becomes alkaline, especially in warm weather." p. 52.

There is almost constantly a frequent and urgent desire of passing urine both by night and day; sometimes a sense of weight or dull pain in the back, and occasional irritation above the neck of the bladder. The functions of the skin appear to be natural; the pulse is not affected; there is no remarkable thirst, or craving for food, nor are the functions of the stomach and bowels much deranged, and the tongue is clean. The subjects were mostly middle-aged men of thin and spare habit, free from gout and constitutional disease in general. In most of the cases sedatives, and particularly opium, have been the most efficient remedies. Purgatives and alteratives were also generally useful. In one case, copaiba decidedly increased the complaint. Dr Prout thinks, that if permitted to proceed, diabetes, or a deposition of the earthy phosphates, may succeed.

Diabetes is the subject of the next chapter; and we agree with our author in the propriety of confining the term to that disease in which the urine contains sugar. With this disease we are better acquainted than with those affections described in the preceding chapters; and our experience confirms almost every thing stated by our author, especially when he differs from the vulgar opinion. We have seen the urine in full fermentation like small beer a few hours after it was made; but did not know till now that it was owing to the presence, in such cases, of chylous matter acting like yeast.

From a very accurate record of a considerable number of cases, we can also confirm our author's statement, that in the best authenticated cases, the reported enormous difference between the quantity of injesta and urine has not been observed.*

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* See also Mynster (C. H.) Observationes de Diabete Mellito. Acta Regiæ Societatis Medicæ Hafniensis, vol. V. 8vo. Hafniæ, 1818.

" 4. Five similar rabbits were fed twice a day. No. 1. with two drachms of barley meal and water; 2. with two ounces of Halle brown beer; 3. with two ounces of Halle white beer; 4. with an equal quantity of a decoction of groats; and No. 5. with a solution of two drachms of sugar.

" The rabbit fed with groats got an increased appetite, and an increased quantity of urine, which was alkaline, instead of acid; and although it had a peculiar heavy smell, it contained no sugar, but a considerable quantity of albumen and mucus.

" The rabbit fed with white beer became in four days depressed, and crept about with difficulty. On the fifth day it died. On dissection, its stomach and part of its intestines were moderately inflamed, the kidneys red, and the urinary bladder empty and contracted.

" The rabbit fed with sugar began to get ill in eight days. Its urine was now alkaline, but contained the usual quantity of urea and no sugar. No sugar could be detected in its blood, either arterial or venous. On the 11th day it died; but, except some inflammation of the stomach and upper intestines, nothing was observed.

" The rabbits fed with barley meal and brown beer continued well.

" *Exp.* 5. A dog was fed twice a day with two drachms of ergotted rye powdered, and water. It became weaker, but seemed to acquire increased appetite and thirst, and its urine was increased in proportion to its drink. It contained a little colouring matter of the blood, but otherwise was natural, and its stools occasionally contained blood.

" Another dog was fed at the same time with a saturated decoction of oats, and suffered no inconvenience. His urine was increased in quantity, but not altered in quality. After having been twelve hours without food, four ounces of diabetic urine were injected into his stomach. His urine, passed three hours afterwards, contained sugar. The same experiment was repeated some days afterwards with the same effect.

" Five ounces of fresh diabetic urine were next injected daily for eight days. During this time the quantity of sugar contained in the urine regularly increased; and after the injection of diabetic sugar was omitted, the secretion of sugar continued, but gradually decreased, and, on the third day, ceased to be perceptible. During the following four weeks, during which he got common food, nothing preternatural could be detected in his urine.

" *Exp.* 6. Diabetic urine was injected twice a day into the stomach of a dog and two rabbits. From the first day sugar was found in the urine of all. In ten days one of the rabbits became depressed, and its urine was scanty. On the 12th day, the urine no longer smelt or tasted saccharine, and contained no sugar, but urea and uric acid, as in the natural state, with an excess of albumen.

It died on the 16th, and was opened while still warm. The thoracic viscera were sound, the stomach empty, contracted, and of a deep red; the intestines covered with a white slime, and filled with the remains of gross food, but otherwise sound, as well as the liver and spleen. The thoracic duct, brain, spinal marrow, and the great nervous trunks of the thorax and abdomen, were quite natural. The kidneys were very red, and full of blood; but the bloodvessels, ureters and bladder, were quite natural. An injection of two drachms of sugar, one grain of carmine, and 1½ oz. of water, was thrown into one of the kidneys, and into a healthy kidney for comparison, with as equal force as possible; and a few drops of the injection came through the ureter of the diseased kidney, but not through the healthy.

"The other rabbit and the dog continued well. On the 18th day they were galvanized, without any sensible effect. On the 23d the experiment was terminated; and during the next two days the quantity of sugar in the urine gradually decreased, while the urea reappeared, and increased until the urine became natural.

"7. *Exp.* A drachm of a solution of the extract of diabetic urine was injected into the jugular vein of a dog. Immediately it became restless, and its heart beat rapidly. In half an hour it was as lively as usual. In two hours its bladder was emptied by pressure, and the urine smelt and tasted quite sweetish sour, sp. gr. 1052, and contained sugar and albumen, but no urea. Next morning its urine was natural. This experiment was repeated with the same result.

"A ʒi. of diabetic syrup was then injected into the jugular vein, a capacious glass fixed before the mouth of the animal, and the femoral vessels laid bare, and in 12 minutes a little blood drawn from each. Traces of sugar were found in the arterial blood only. In 30 minutes a little more blood was drawn, but now neither arterial nor venous contained sugar, which was however found in the urine; and from the smell and taste, was conjectured to be in the vapour of the breath.

"This experiment was repeated; but after injecting the diabetic syrup, the animal was galvanized, and now sugar could not be detected either in the arterial or venous blood, or in the urine, although the vapour of the breath had still the sweetish smell and taste.

"*Exp.* 8.—Two drachms of fine cane-sugar syrup were injected into the jugular of a dog, but no sugar could be detected in the blood, either arterial or venous, or in the urine, but the expired vapour seemed to be sweetish.

"*Exp.* 9.—The urinary organs of a dog were strongly galvanized, which excited thirst, a more copious secretion of a turbid, heavy urine, containing much albumen and some red particles of the blood. After some time, a weaker degree of galvanism restored the natural condition of the urine, and the healthy state of all the functions.

"*Exp.* 13.—Two drachms of fresh calf's bile were injected into the jugular of a dog; and in half an hour the urine had a deep, yellow colour, and it appeared to contain bile, as it exhibited exactly the same phenomena with reagents as heathly urine, with which bile was mixed intentionally. This injection of bile was repeated twice a day, for ten days, during which time the animal continued well, and the urine always contained bile, but not a trace of sugar or urea. In two days more the urine returned to its natural state, and contained urea, but no bile.

" *Exp.* 14.—Bile was injected into the jugular of a dog. In two hours the conjunctiva was yellowish, and the urine altered; and in the blood, both venous and arterial, there was bile. When in this state, galvanizing the animal did not restore its functions to the healthy state, as in the experiments with saccharine and anylaceous food.

" *Exp.* 15.—The hepatic duct of a dog was tied, which produced a state resembling jaundice in man. As the bile increased in the urine, the urea disappeared, and the animal died on the third day of rupture of the duct."

Dr Krimer admits that his experiments have not advanced our knowledge of diabetes very materially. But they show, in his opinion, that certain kinds of grain, viz. rye, ergotted rye, oats and rice, diminish the activity of the nervous system, and of the *par vagum* in particular, whereby the urine is rendered very dense. Its usual constitutents, urea and uric acid disappear, and their place is supplied by albumen and the colouring matter of the blood. The difference of effect of cane sugar and diabetic sugar, when injected both into the stomach and into the jugular, is also worthy of notice. It further appears, that the secretion of urine partly depends upon the *par vagum*, and that a diminished action of this nerve produced an increased quantity of solid matter in the urine, and though not of sugar, at least of albumen, mucus, and red particles of blood, and that diabetes might, in like manner, depend upon a similar state of the *par vagum*.

Lastly, Dr Krimer observes, that his attention was first directed by Professor Nasse to the probable relation of the secretion of bile to diabetes; and this relation seemed to him more worthy of notice, as in many respects there is an analogy between the secretion of bile and urine. With the appearance of bile in the urine, in bilious diseases, the urea disappears in the urine, in the same way as it does in diabetes, in proportion as the sugar increases; and the experiments of Berzelius show, that in bile in the healthy state, there is a saccharine matter. Now, concludes Dr Krimer, may there not exist in diabetes such a modification of the vital action, that the saccharine matter of the bile is determined to the kidneys?

With respect to the treatment of diabetes, our author very properly distinguishes between its acute and chronic state. It is in the former only that any advantage can be expected from venesection, and even in that we are sorry that our experience does not confirm the expectations raised by the reports of Dr Watt. We must also remark, that V. S. is not an indifferent operation in diabetic habits. The two first cases of diffuse inflammation of the cellular tissue, which Dr Duncan junior observed, were both diabetic patients, * which confirms what Dr Prout observes, that he thinks he has remarked, "that individuals who are subject to urinary derangements, are apt to be liable to that dangerous affection termed *diffuse* inflammation, which consists in a gradual extension of inflammatory action, from a trifling wound over the whole system." p. 139.

Dr Prout also keeps in view another practical distinction between diabetes, as a simple saccharine condition of the urine, and as complicated with increased quantity of the secretion. Against the former he thinks it exceedingly doubtful, if any specific remedy exist, and he places little reliance on a diet exclusively animal. He says he has watched its effects in diabetic patients. In most instances it seems to lessen the quantity and deepen the colour of the urine, and thus to disguise the saccharine matter present; but as far as he has been able to ascertain, it does not diminish the specific gravity of the secretion; and indeed the alternation of an excess of urea with sugar, already alluded to, is quite destructive of our hopes of curing diabetes by azotized food. At the same time, there is no doubt that, in some instances, the disease may be suspended by animal diet, and reproduced by a very slight error in diet. A nobleman in this neighbourhood, by great attention to diet, kept the disease long in check, but being a keen horticulturist, could not occasionally resist the temptation of merely tasting new varieties of fruit, although an aggravation of the disease always followed.

We have not had occasion to remark sudden death from excess of food. Our patients died either in an extreme state of emaciation, or of accidental diseases; but we entirely concur with the dietetical regimen prescribed by our author, and can confirm his caution as to the serious consequences which have been known to follow a sudden and great diminution in the quantity of food. A patient, whose drink and urine had for some time time been steadily about ℔. xiv. daily, was recommended to abstain as much as possible from drink. But although he did not reduce his drink lower than ℔. ix. or x. the attempt proved fatal; he was attacked with headach and giddiness, nausea, loss of appetite; the giddiness rapidly increased, his vision and hearing became impaired, his gait unsteady, his speech indistinct, and he soon sunk completely exhausted. On dissection, the brain seemed not to fill the cranium; and it excited the idea that the absorbents, not being supplied with extrinsic fluid to satisfy their activity, had preyed upon the body itself. Dr Prout recommends that all drinks should be taken in a tepid state, and when the stomach is not loaded with solid food. Of remedies, sedatives, and especially opium, appear most beneficial, but mercury, and all active diuretics hurtful.

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* Transactions of the Medico-Chirurgical Society of Edinburgh, Vol. I. p. 474.

Our author next proceeds to consider those diseases of the urine, in which principles insoluble in that secretion are morbidly deranged in quantity or quality; and he divides the deposites which occasionally take place in urine, into three kinds; the amorphous, the crystallized or gravel, and the solid concretions or calculi. The amorphous sediments are indicative of an excess of lithic acid, and consist essentially of lithic acid combined with a base, generally ammonia. They vary in colour, being yellow, or red, (the lateritious sediment), or pink. The yellow are the sediments of health, the red indicate feverish or inflammatory action, rather going off than present; and our author ingeniously explains their occurrence almost constantly in some cases of continued fever, by supposing that the sediment of to-day was generated by the fever of yesterday, and those generated to-day will appear to morrow. They are very decided in gout, rheumatism, and hepatic affections. The pink indicate general fever of an irritable nature as hectic, and occurs in the urine of dropsical individuals, and of those labouring under certain chronic visceral affections, especially of the liver. The colour of these deposites depends upon two substances; the first an ingredient of healthy urine, which forms the yellow deposites, and the other purpuric acid, upon which the pink sediment depends, while the lateritious is a mixture of both.

The crystallized deposites are also of three kinds; of lithic acid, by far the most frequent, and always red; the triple phosphate of magnesia and ammonia, always white; and the oxalate of lime, extremely rare, of a blackish-green colour.

The solid concretions, although presenting numerous varieties, are made up of four elementary substances only;—lithic acid and its compounds; oxalate of lime; cystic oxide and the earthy phosphates. Dr Prout has entered with considerable minuteness into the comparative frequency of these, and has drawn up a detailed table from the observations of Mr Brande, Dr Marcet, Dr Henry, Mr Smith, and at Guy's Hospital. We abstract the general total.

Lithic acid	294	Alternating calculi	186
Mulberry	113	Mixed compound	25
Phosphates	3	Grand total	823

The observations on the different kinds of calculous diathesis are truly practical, but scarcely admit of abridgement. The lithic acid diathesis is the most frequent. Children, and especially those of dyspeptic and gouty parents, are exceedingly liable to lithic acid deposites, not only amorphous but crystallized.

> "In this case symptoms of irritation about the urinary organs may be always more or less observed, if the child be attended to. Thus there will be found to be a frequent desire to pass urine, which is voided in very small quantities, and with manifest uneasiness. The irritation about the urinary organs also frequently induces the child to wet the bed by night, &c. In such cases, if the urine be examined, it will be always found to be very unnatural, and frequently loaded with lithic acid; and should this prove to be the fact, the case requires immediate attention, as there is much greater risk at this period of life than at any other, of the formation of stone in the bladder." pp. 131, 132.

Between puberty and forty, there is less tendency to lithic acid deposite than at any other period of life. About forty, lithic acid is apt to be discharged in larger quantities than usual; and for some time previous to the occurrence, there is more or less derangement of the general health, often in the form of various anomalous nervous affections and pains, accompanied by great derangement of the digestive functions, which will suddenly obtain relief by a discharge of lithic acid gravel, or a renal calculus. After sixty the urine is apt to become neutral, and earthy phosphates are liable to be deposited.

The precipitation of lithic acid depends upon the presence in the urine of a free acid, commonly the muriatic, sometimes the phosphoric or sulphuric, and occasionally other acids, which act by decomposing saline compounds and setting a destructible acid free, which is the immediate cause of the deposition of lithic acid gravel. When the urine contains a free acid it is commonly more transparent than usual, and of a bright copper colour.

It is in the lithic acid diathesis that alkaline remedies or neutral salts containing a vegetable acid, are for the most part particularly indicated, conjoined especially with alteratives and purgatives; and the use of hard waters is particularly hurtful. But to do any good, the treatment and regimen must be continued for a considerable length of time. In some cases of general constitutional derangement, great relief is obtained by bringing about a sort of artificial crisis, by exciting the kidneys to secrete an unusual quantity of lithic acid. This is done by laudanum conjoined with certain active diuretics, as turpentine, or muriatic acid and turpentine, or, what is safer, potash and turpentine.

When the oxalate of lime diathesis exists, Dr Prout thinks that he has seen considerable advantage, by endeavouring to change it into the lithic acid diathesis, by the exhibition of muriatic acid, until the lithic acid began to be plentifully deposited by the urine in cooling.

The cystic oxide diathesis is so rare that little certain is known regarding it.

The phosphatic diathesis is very seldom an original affection, but is commonly induced by the deposition of lithic acid or oxalate of lime.

" A deposition of the earthy phosphates from the urine has been long observed to be attended by very distressing symptoms, though no one seems to have hitherto generalized them. They consist in great irritability of the system, and derangement of the chylopoietic viscera in general; such as flatulency and nausea, obstinate costiveness, or peculiarly debilitating diarrhœa, or both frequently alternating; and the stools are extremely unnatural, being either nearly black, or clay-coloured, or sometimes like yeast. These are always accompanied by more or less of a sensation of pain, uneasiness, or weakness, in the back and loins. There is a sallow, haggard expression of countenance; and as the disease proceeds, symptoms somewhat analogous to those of diabetes, begin to appear, such as great languor and depression of spirits, coldness of the legs, complete anaphrodisia, and other symptoms of extreme debility; and the disease, if not speedily checked, seems capable of ending fatally. The urine in this form of disease is invariably pale coloured, and, upon the whole, voided in greater quantity than natural. Sometimes (generally, I think, by day), it is voided in very profuse abundance; and in this case is of very low specific gravity, 1.001 or 1.002, for example. At other times it is voided in less quantity, and its specific gravity is proportionally higher, but it is seldom very high, that is, surpassing 1.025. In the former case it is generally perfectly pellucid and colourless, and deposits no sediment; in the latter, it is sometimes opake when passed, and always after standing for a greater or less time, deposits a most copious precipitate of the mixed phosphates, in the state of an impalpable powder. In all cases the urine is extremely prone to decomposition, becomes alkaline by the evolution of ammonia, and emits a most disgusting smell." pp. 178, 179.

In this condition the particular indications of cure seem to be to diminish the unnatural irritability of the system, and to restore the state of the general health by tonics and other appropriate remedies. The first is done by the free use of opium; and when the distressing symptoms are relieved, the mineral acids, cinchona, uva ursi, and different preparations of iron and other tonics, may be employed in conjunction with opium. Alkalies and salts of vegetable acids, and mercury, are to be avoided; and animal diet seems preferable to acescent food.

The second part of this valuable Treatise is on the mechanical and organic diseases of the Urinary Organs; and as these are chiefly surgical, our author cannot be expected to enter so fully into them; yet, every chapter evinces extensive knowledge and an inquiring mind. The following theory of the formation of renal calculi, is ingenious and original.

"The kidney is made up of a congeries of similar parts, or little kidneys, if we may use the expression, each one of which is independent of the others in its structure, and may therefore, probably, independently of the others, become more or less deranged in its functions. Let us suppose one or more of these little kidneys similarly deranged to the others, but in a greater degree, so as to secrete very little water, but a large proportion of lithic acid. In such a case the lithic acid must be obviously separated in that peculiar semi-fluid condition, or state of hydrate. Lithic acid is well known to be capable of existing in a sort of semi-fluid state, or as a *hydrate*, for some time before it undergoes the process of crystallization. This may be illustrated by dissolving a little of this principle in an alkaline solution, and precipitating it, when cold, by the addition of muriatic acid. The lithic acid separates in the form of a bulky gelatinous mass, which, after a greater or less time, begins to diminish rapidly in magnitude, and at the same time to assume the crystallized form. The lithate of ammonia voided by birds, serpents, &c. and the lithate of soda formed in the human subject during gout, when first secreted, exist in this semi-fluid or plastic state, and afterwards become hard, apparently by undergoing an imperfect kind of crystallization, by which they are separated from the water with which they are combined and held in imperfect solution. I have repeatedly seen both the lithic acid and lithate of ammonia deposited in the urine in this state of hydrate under the form of a gelatinous mass, having much the appearance of mucus, for which I at first mistook it.

"In this state it is bulky, and may thus occupy the whole of the infundibulum in which it has been deposited; or the quantity may be supposed to be sometimes so great as to be partly protruded, in a similar state, into the common receptacle or pelvis of the kidney. After remaining in this state for a greater or less time, crystallization may be supposed to take place; the semi-fluid mass will now be much diminished in bulk, and perhaps reduced to the form of a congeries of crystals easily separable from one another, and thus pass off in the form of gravel; or, what may easily be supposed to take place, (especially when the lithic acid is very impure, and combined with a larger portion of other matters than usual), it may assume the form of an imperfectly crystallized or amorphous mass, and thus constitute a nucleus possessing these characters; or something between these two extremes may take place—the plastic mass may separate partly into crystals, and partly remain an amorphous mass, enveloping those crystals; in which case a mixed kind of nucleus will be formed." p. 208.

The following observations on the state of the urine in the different species of vesical calculi are instructive. The urine with lithic acid calculus is always of the natural colour. Its sp. gr. is higher than that of health, and it almost always deposits crystallized sediments on cooling, which are commonly much increased at periods when the pains and irritation are worse than common. At these times also, the crystallized sediments are not unfrequently accompanied by amorphous sediments and mucus. When, however, the stone is of small or ordinary size, the mucus is generally by no means so abundant in this species of calculus as in some others; and the urine, which is sometimes a little turbid at first, commonly becomes, after standing some time, perfectly transparent.

The urine, in calculus of oxalate of lime (abstracting the mucus and blood with which it is frequently more or less mixed), apparently differs little from healthy urine; that is to say, it is acid, and free from all gravelly deposites, except perhaps occasionally a little lithate of ammonia.

In cystic oxide culculus, the urine was copious, of a yellowish green colour, and strong peculiar smell, sp. gr. about 1020. It faintly reddened litmus paper; and almost immediately on being passed a greasy film was formed on the surface, and at the same time rather a copious pale-coloured precipitate appeared, and the urine became alkaline. The film and precipitate consisted chiefly of the triple phosphate of magnesia and ammonia, mixed with a little of the cystic oxide; and on the addition of acetic acid, a considerable proportion of cystic oxide was precipitated. The urine contained very little urea, and hardly a trace of lithic acid.

With the phosphatic calculi the urine is highly characteristic. It is generally voided in considerable quantity, and is of a pale whey colour, and slightly opaque. Its sp. gr. is low, from 1006 to 1012. It commonly deposites the phosphates in abundance, intermixed with a large quantity of a peculiar mucus, having a a very characteristic appearance. The urine soon becomes alkaline, and undergoes the putrefactive process, and in this state emits a most offensive smell.

Prostatic calculi consist essentially of phosphate of lime, and the quantity of the substance secreted by this gland is sometimes immense.

In this chapter Dr Prout considers also other diseases of the kidneys, bladder and prostate, and the method of treating them.

The next chapter gives a view of the comparative frequency of calculi at different periods of life, on the authority of Mr Smith's accurate and valuable table, of Dr Marcet, and the records of the Leeds Infirmary. The result is 509 of 14 years and under, and 549 above 14; of whom 1014 were males, and only 44 females.

In determining the cases in which lithotomy is advisable, Dr Prout recommends it in all instances before puberty, of whatever species the calculus be, and whenever the phosphatic diathesis is certainly present; and he thinks it may be postponed in adults whenever the calculus is of small size, and the lithic acid diathesis is steadily present, particularly if the patient be in the prime of life, the constitution sound, and the sufferings comparatively moderate, and provided the patient will conform to the proper regimen.

The work is concluded with a most valuable recapitulation, containing practical rules for determining the nature of the affection and its appropriate remedies, from the properties of the urine and other symptoms. We would willingly transcribe it, but we must not trespass longer on the patience of our readers, than to recommend it to be read and studied with the greatest attention.

The different leading varieties of calculi are well represented in a coloured plate containing seventeen figures, and the methodic arrangement in which they are grouped, indicates an highly commendable attention to minute particulars, which, in the example of our author, we see is not incompatible with an enlarged mind.

II.

A short Inquiry into the Capillary Circulation of the Blood; with a comparative View of the more intimate Nature of Inflammation: and an Introductory Essay. By JAMES BLACK, M.D. and S.R.N., &c. London. LONGMAN, &c. 8vo. pp. 176. 1825.

THE work of which we are about to give a short account, is on a subject which has occupied the attention of the first physiologists in every age of medicine, and concerning which opinions have scarcely been at any time more divided than at the present moment. The fascinating influence of a great name had long preserved an undisputed sway in the schools of Britain to the doctrines of Hunter regarding the physiology and pathology of the circulation. But the experimental inquiries of our countryman Dr Thomson, and the later discoveries of Dr Philip have at least rendered doubtful many of the particulars of Hunter's doctrines; and at present the mind of the medical public, if we except a few of his immediate disciples, is well prepared to receive with impartiality any new facts or arguments on either side of the controversy.

To an inquirer, who, like our author, professes to aim at no new discovery, but to give to the world the results of researches made chiefly with the view of satisfying his own mind where the truth lay between the several discrepant statements and contending opinions of physiologists, we presume the attention of his brethren is peculiarly alive. And we may venture to assure them, that their willingness to listen will meet in the present instance with an ample recompense. For his experimental researches, while they bear internal evidence of accurate observation, faithful description, and impartial judgment, are intrinsically of no mean value.

As our object is not to analyze the whole work, but to give a brief account of those parts of it only which relate to the abovementioned controversy, we shall pass over his Introductory Essay. It is a well argued exposition of the advantages of physiological study to the practitioner, and of the relative character and resources of the physiological and experimental physician.

The Second Essay treats of the Capillary Circulation, and consists of an argument in favour of an independent action in the capillary vessels drawn from the principles of mechanical philosophy, and from microscopical observation on the state of the capillary vessels in health and disease.

He first endeavours to show, and in this he appears successful enough, that, without some action in the capillary vessels, it is impossible to conceive how the circulation in them can be carried on. Although there is nothing particularly new in the method by which this is proved, yet as it is done succinctly and logically; and as it points out some very gross violations of the principles of mechanics, which have been committed by those who have argued the opposite doctrine, we shall notice his proof shortly. In the first place, it is impossible to conceive that the heart's contractions are alone sufficient to carry on the capillary circulation. The mean power of the heart, or the mean force with which the blood is impelled into the aorta, is only equal to three pounds on the square inch. Now double that force, or six pounds on the square inch, is required to produce a continuous stream of water through a tube one-hundredth of an inch in diameter. But on the one hand, the extreme capillaries have a diameter at least ten times less, and on the other, the viscidity of the blood is such as to require about four times as much power as water for its transmission through tubes of equal calibre. These data, therefore, if correct,—and they are all of them derived from strict experiment and observation,—render it quite evident, that the heart's action alone is not nearly sufficient for propelling the blood through the capillaries. Dr Black next proceeds to show that the contractions of the heart are not aided by contraction of the greater arteries. He does not enter into the disputed point of their contractile or muscular power; but submits, that even granting them to possess such a power, the concession will not bear the inference to be founded on it, that the progress of the blood is in consequence accelerated. For contraction of the arteries would have as great a tendency to make the blood recede as advance; and it could not act as a propelling force, "without using the column of blood next the heart as a fulcrum, and so detracting from the free force of the heart itself." It is clear therefore, upon fair mechanical principles,—independent of the facts which point out an occasional local increase in the activity of the circulation,—that the capillaries must possess some intrinsic action by which the blood is traasmitted through them.

Our author next goes on to prove, that this intrinsic action of the capillaries is an independent one; that it may be carried on without that of the heart; and that, although susceptible of impression through the nervous system, it does not depend on the brain or nerves for its existence.

The last of these statements is fully made out by the capillary circulation continuing when the communication with the rest of the body by the nerves is destroyed; and the impressibility of the capillaries through the medium of the nervous system is shown by their circulation ceasing suddenly when the brain is crushed, or its functions annihilated by concussion.

The independence of the capillary action in regard to that of the heart is by no means so clearly established. On the whole, Dr Black's microscopical observations tend rather to confirm the opposite doctrine. For, like Haller, he could not observe that the capillary circulation went on after the heart was cut out, or the aorta tied. Yet he himself allows that his experiments on this point were defective, and are not to be held as outweighing those of Dr Philip, who on one occasion observed the blood circulating rapidly in the capillaries, even seventy-five minutes after the heart had ceased to beat.

There is no room for doubting, however, that the circulation of blood in the capillaries may be affected by causes which operate neither through the heart nor through the nervous system, but directly on the capillary vessels themselves; and consequently that, to a certain extent, their action is independent of that of the heart, though it may not be so altogether. The experiments of Dr Black on this head support, in the most direct and pointed manner, those made not long ago by Dr Thomson, Dr Philip, and others; and render our knowledge more precise in some particulars regarding the changes the capillaries undergo, when affected by extraneous agents.

If the capillary vessels of the web of the frog or duck's foot be observed through the microscope when stimuli are applied, it is remarked that under certain stimuli, such as ammonia, oil of turpentine, or mechanical irritation, the vessels, after a few minutes contract upon themselves, and sometimes even to such a degree as to expel and deny entrance to the red globules, and consequently to become invisible. But in no long time, the propulsion of the blood in the greater arteries leading to the part appears gradually to distend the contracted vessels; the distention soon surpasses the natural standard; and the part becomes redder, apparently more vascular, in short congested. Some stimuli are so slight that the contraction is trivial, the subsequent dilatation also insignificant, and the restoration of the healthy state of the vessels rapid and complete. Some other stimulants, such as a solution of salt, produce enlargement without previous contraction. Whenever the vessels contract, the blood moves with increased velocity; and on the contrary, when they enlarge, it becomes retarded, so as sometimes even to stagnate altogether. Two deductions may be drawn from these facts; in the first place, that the capillaries have at least one kind of action completely independent of the heart and vessels; and, secondly, that the ultimate effect of the application of a strong stimulus to them, is to take away some property or other, which they possess over and above the properties attached to their physical texture, and by which they resist in their natural state the propulsive and distending power of the blood.

By what particular property the capillary vessels act in propelling the blood our author does not pretend to determine. He most properly shows, that capillary attraction must go for nothing in the exercise of their functions, as that would tend to retain merely, not to transmit the blood. It is farther agreed on all hands, that the disputed muscular coat of the larger arteries, even granting its muscularity, does not form a covering to the capillaries, and consequently that it can hardly be by muscular contraction that the blood is propelled in them. But the precise nature of their action cannot at present be decided.

Leaving the field of observation and fact, and drawing a distinct line of boundary where he leaves it, Dr Black then proceeds to state his notions of the state of the capillaries under the impression of stimuli. He considers their natural state, or their mean state between the contraction produced in the first instance by stimuli and the dilatation that subsequently ensues, to be the result of a property, which he terms *tensiveness* or *erectile* power, but the nature of which he seems to be much at a loss how to explain. The contraction caused by applying a stimulus is the consequence not of a vital constriction or spasm, but of the loss or diminution of the tensiveness of the vessels, resulting from an exhaustion of their *materia vitæ*, or vital principle; and their subsequent dilatation is owing to the impulse of the blood acting on tubes temporarily or permanently deprived of every power of resistance, except that feeble one derived from their physical texture. The latter part of his explanation of the phenomena is easily understood; but we confess we are at some loss to comprehend how the contraction of the vessels can arise from the mere negation of *tensiveness*,—particularly when he tells us that some stimuli cause dilatation without any previous collapse or contraction.

In the Third and only remaining Essay, Dr Black treats of the nature or proximate cause of Inflammation. As may be anticipated from what we have said of his microscopical inquiries, he joins the ranks of those who maintain that inflammation depends not on increased action, but on debility, or deficient energy of the vessels of the affected part. And he is no mean auxiliary; for his belief is founded on a fair basis of fact; and perhaps the cause he espouses has not hitherto been so well argued as in his Essay.

He sets out in his exposition by tracing the nature of certain healthy actions connected with increased afflux of blood, such as flushing of the face, and swelling of the papillæ and penis. Very slight stimuli, it has been seen, cause a trivial atonic collapse, followed by dilatation of the capillaries, and soon afterwards by their return to their natural condition. The emotion which causes blushing acts in this manner. It is a sedative emotion, causing slight atony in the capillaries, followed immediately by dilatation and redness, if the heart is not affected at the same time, but in the first instance by paleness, if the emotion is so strong as to affect the heart's action also. The stimuli which cause intumescence of the papillæ or penis, act on parts which, when at rest, are in a state of collapse; the stimulus, whether sensorial or extrinsic, throws the collapsed vessels into a state of tensiveness; and "the approaching blood, solicited to the part from the topical increased capacity, adds to the accumulating vital power, in proportion to the extent of the turgescence." In this part of his exposition, he seems to us to depart altogether from that part of his theory which supposes that stimulants exhaust the capillary tensiveness.

Betwixt the state of the capillaries now mentioned, and that occurring in perfect inflammation, there is an intermediate affection of them, the nature of which, if well understood, would throw much light on the process constituting true phlogosis. *Irritation,* however induced, is characterized by certain uneasy feelings in the part, and by increased vascularity. It arises from the constant or repeated application of some stimulus, producing a diminution in the tensiveness or resisting power of the capillary vessels, and consequently dilatation of them, but without stopping the circulation through them, and without totally annihilating their office in the process of nutrition. Under this theory are explained satisfactorily the redness of the affected part, and the frequent formation of morbid growths,—which are the chief phenomena of the state vaguely expressed by the name of Irritation.

The state of the vessels in *Inflammation* is similar in nature, but superior in degree to that which constitutes irritation. The capillaries are more dilated. And for that end it is necessary that their tensiveness be more completely impaired by the greater exhaustion of the *materia vitæ.* This is brought about either directly by the application of sedatives, or indirectly by the exhaustion which follows that of strong stimulants. The process constituting simple active inflammation in its first stage, therefore, is collapse or atony from too great stimulation, then turgescence of the capillaries from the impulse of the blood, and slow transmission of the blood through them, or actual stoppage of it. This state of the capillaries and blood was seen very distinctly in the web of a frog, on which there was an ulcer surrounded by much inflammation. "On being brought to the microscope, the inflamed circle was observed to consist chiefly of enlarged capillary veins, the blood in which was circulating very sluggishly, but was nowhere stagnant." On applying oil of turpentine, "the current in the vessels round the ulcer became more and more sluggish, until complete congestion formed all round the margin."

All the phenomena of the first stage of inflammation are evidently explicable on the foregoing theory. The *swelling* is owing in the first instance to the dilatation of the capillaries, and the consequent afflux of a greater quantity of blood. The *redness* is owing to the same cause. More blood is contained in each capillary that formerly contained red globules, and possibly capillaries contain red globules which formerly did not admit them. The redness is *arterial* in colour, although the chief part of the dilated vessels are veins, on the one hand because the blood naturally does not acquire its proper venous colour till it has left the minuter capillary veins, where probably it is in part elaborated into the various secretions,—and on the other hand, because the nutritive processes usually carried on in the capillaries, and to which the de-arterialization of the blood is owing, are suspended. The sense of *heat* arises from the greater quantity of blood sent into the capillary system, and to its superfluity not being carried off by the secretions. *Pain* is owing to the vessels yielding to the distending force of the blood, and pressing on the delicate nervous fibrils in their neighbourhood; and the *throbbing pain* which accompanies some inflammations is evidently caused by the dilated vessels transmitting more distinctly the impulsive force of the heart's contractions.

In what is called chronic inflammation, the state of the capillaries is nearly the same as in the acute variety; only the atony is less in degree. Hence the nutritive processes can be in part carried on, as is fully proved by the blood undergoing partly its usual change, and by the frequent formation of foreign growths.

The action of remedies admits of an easy and obvious explanation on the foregoing principles. Depletion acts by diminishing the impulsive force of the heart and lessening the quantity of blood in the capillaries, so as to permit the operation of such natural causes as tend to restore their tonicity. In strong healthy persons, when the violence of the inflammation is owing more to the increased impetus of the blood, than to the loss of tensiveness or tone in the vessels, depletion alone is often adequate to the cure. But in persons of weak habit, in whom the tensiveness of the vessels is more thoroughly destroyed, and the restorative powers are more feeble, the effect of depletion is only to remove the derangement of the general circulation, and not that of the capillaries also. In order to remove the morbid state of the capillaries, it is requisite to employ likewise artificial stimuli, in order to aid the natural powers in restoring to the vessels their tensiveness and natural state of contraction.—Sometimes local stimuli will remove inflammation without a remedy being applied to the excited state of the general circulation. This evidently depends on their causing the dilated vessels to contract, the stimulus being at the same time of such a nature as not to exhaust the *materia vitæ* in the part, and so bring on subsequently atonic dilatation. Such local stimuli must likewise be free of all influence on the heart, otherwise the impulsive force of the blood is increased; which does away with all the advantage gained by the increased tone or tensiveness of the vessels.

It is an error, our author continues, to suppose, that in inflammation the arteries leading to the part act with increased force. For, in the first place, if we are to credit the growing and well grounded belief of modern physiologists, the arteries have not an independent power of action; and, secondly, if they had, and their action did increase, they would contract, not dilate, as they really do. Their state, therefore, when they are affected at all, will be rather one of *asthenia*, propagated in fact from the capillaries by the sympathy of continuous surface.

The heart alone is generally in a state of increased action; but our author does not enter upon the process by which this is produced, reserving it for the subject of a future dissertation.

The imperfect sketch now given of Dr Black's treatise will probably satisfy our readers, that his views are ingeniously stated and for the most part legitimately deduced from his own experiments and those of his distinguished predecessors. There is one of his tenets, however, which appears to embarrass him not a little, and introduces obscurity and confusion into several parts of his Essay; namely, that in which he assumes that the contraction caused in the capillaries in the first instance by various stimulants is a state of atonic collapse. If it is so, it appears difficult to conceive how, in inflammation, any good can arise from the use of local stimuli; since they could not cause contraction without still farther lessening the tone or tensiveness of the vessels. Accordingly, in treating of that part of the subject, he assumes that local stimuli increase or restore their tensiveness; an assumption which is evidently at variance with the tenets laid down in the previous part of this work.

His experiments and arguments, however, are on the whole close and conclusive; and there can be no doubt that he has ably supported the doctrine which is now gaining ground rapidly,—that the essence of inflammation is loss of tone and depressed action in the capillary vessels.

III.

Observations on the Transfusion of Blood; with an Account of two Cases of Uterine Hemorrhage, in which that Operation has been recently performed with success. By CHARLES WALLER, Member of the Royal College of Surgeons, London, &c. London, 1825. 8vo, pp. 34.

THE operation of transfusion, introduced into surgical practice with so much pomp towards the close of the seventeenth century, and from that time neglected till its revival not many years ago by Dr Blundell, has of late attracted several zealous adherents in the English capital. The pamphlet before us is composed by one of its admirers, who has introduced it into regular practice, and in his own opinion with great success.

In estimating the trust-worthiness of a writer on a favourite point of practice, to which there is a natural repugnance in the public generally, and which by the profession is viewed with fear and treated sometimes with sneers and persecution, much will depend on the tone of moderation and impartiality in which he writes. We need scarcely add, that a great proportion of the present Journal and Pamphlet writers in medicine and surgery fail altogether in meeting with the confidence of their brethren, in consequence of their writings being tinged with the opposite qualities of inflation of style, partiality of narrative, and exaggeration of sentiment.

Mr Waller, however, has shown much judgment in these respects. For his narrative is simply and neatly told, the risks he ran are not concealed, and he rests satisfied with proving that the practice is applicable to the particular object for which it was actually applied. On these accounts we have read his pamphlet with confidence, and may with confidence recommend it to our readers.

The first part of his paper, which relates to the history of transfusion, may be omitted for the most part in this short notice, as the origin and progress of the operation are pretty generally known. A single quotation only may be transferred, as it shows that the operation was at least known, though perhaps not practised, long before the days of Dr Lower and M. Denys, between whom the merit of its discovery has been commonly believed to lie. The following passage, from a treatise by Libavius, published in 1615, proves that that author was acquainted with its practicability half a century before Dr Lower first practised it on animals. "Adsit Juvenis robustus, sanus, sanguine spirituoso plenus: adstet exhaustus viribus, tenuis, macilentus, vix animam trahens. Magister artis habeat tubulos argenteos, inter se congruentes; aperiat *arteriam* robusti, et tubulum inserat, muniatque; mox et ægroti arteriam findat et tubulum femineum infigat. Jam duos tubulos sibi mutuò applicet, et ex sano sanguis arterialis, calens, et spirituosus saliet in ægrotum, unàque vitæ fontem afferet, omnemque languorem pellet."

In making up our mind as to the precise value of this operation in individual cases, there is a difficulty which must always occur, and which Mr Waller has not lost sight of. It is, that in hæmorrhage, either after parturition or in any other state of the body, recovery may take place under the most desperate circumstances, through the mere restorative powers of the system aided by slight occasional stimulants; and hence that it is hardly possible to assign to any given remedy its exact share in the final result. Accordingly, a case came under our notice not long ago, in which the state of the patient was thought to call for the operation, but which ultimately recovered without it, as the surgeon who was invited to perform it believed it to be unnecessary. The opposers of the operation, therefore, will probably consider themselves quite entitled to think that Mr Waller's two patients would have recovered without it.

This much, however, may be said on the other hand; namely, that it is hardly possible to conceive cases in which it was more decidedly called for,—that at the time of the operation the patients were probably in as exhausted a state as they could well be, without death supervening almost immediately,—and that, from the instant the blood was introduced, their state improved rapidly and permanently, although the most powerful stimulants had been previously tried with but very slight and transient advantage.

The circumstances of the cases were shortly as follows:—

A woman of a weak, scrofulous habit had excessive hemorrhage after delivery. When Mr Waller saw her half an hour after the birth of the child, the cheeks and lips had a leaden hue; the breathing was very slow; the pulse excessively languid, sometimes imperceptible; and she had the general appearance of one already dead. Powerful stimulants, such as brandy and ammonia, were frequently given for an hour and a half; but they caused only a slight and temporary excitement. Mr Waller was then satisfied that transfusion was the only remedy which promised any chance of recovery; but, in order to be still farther assured of its necessity, as an occasional amendment could be observed in the woman's state, he watched her for three hours and a half longer. No permanent improvement having taken place after that interval, the operation was performed with the assistance of Dr Blundell. The blood used was taken from the vein of her husband, and the quantity introduced each time was two ounces. After the first portion was introduced, the pulse became somewhat irregular. After the second, there was slight and transient retching and restlessness; but the woman soon expressed herself better, and the pulse, from 120, fell to 110, and became more distinct. In six hours it had fallen to 100, and expanded considerably, and she felt much more comfortable. From this time, slight nourishment being liberally allowed her, she recovered rapidly without a single unpleasant symptom. The wound made by the operation healed slowly; but the vein did not inflame.

The subject of the second case was that of a stout Irishwoman, who had lost a great deal of blood on account of an adhesion of the placenta, and was, if possible, in even a worse state than the other. The pulse had left the wrist; the countenance was bloodless; the sight indistinct; the breathing hurried, and attended with sighing; and brandy, ammonia and laudanum had been freely given, with very little and very transient benefit. The operation was therefore resolved upon; but it could not be performed for some time, by reason of the patient's resistance. At length, however, she consented, and it was performed when she lay in so low a state, that her husband thought her dead. The operator was Mr Doubleday, a friend and colleague of our author, and Dr Blundell assisted him. " At the end of the first injection," which amounted to two ounces, as in the former case, " the pulse was more perceptible, the second it was broader, at the third a very marked improvement was observed, she herself stating that she was as strong as a bull; at the fourth, she said that she felt the blood running up her veins, and that she was quite recovered." Three more injections were employed, making in all fourteen ounces. The pulse had now fallen from 140 to 104; and in less than two hours it descended to 90. In an hour after the operation she could sit up a little, and appeared " as well as women usually are when nothing particular has occurred; " and she continued to improve pretty steadily. A slight feverishness indeed ensued; she was occasionally restless, and the hypogastrium became painful. These symptoms, however, had no connexion with the operation; and they disappeared very soon. This patient had slight inflammation of the wounded vein, which Mr Waller attributes to her having begun too early to attend to her household affairs. It gradually subsided, and was never alarming.

We see no reason to doubt, on considering these abstracts, that the operation produced an immediate and extraordinary improvement; and it is at least very probable that without it both patients would have perished. The only circumstance in the narrative, which may make some people sceptical, is the very small quantity of blood required to bring about the favourable change. But this will not appear so very extraordinary, when it is considered that, for some hours before, the heart had been acting on a greatly diminished volume of blood, and was consequently well prepared to receive the stimulus of a slight addition to its quantity.

The chief, perhaps the sole objection to the remedy, is the risk of inducing inflammation of the vein. This risk would appear at first sight to be considerable, as the vein must be subjected to rather rough treatment. At the same time it should be remembered, that the inflammation, if induced at all, will exist under circumstances peculiarly favourable to its cure. Perhaps Mr Waller and his friend did not follow the plan best fitted for obviating the risk of inflammation. They introduced the pipe of the syringe into the vein directly, and of course had to withdraw it at each injection, unless they used a syringe with a double tube, which is not mentioned. We apprehend, that much less irritation would be caused by an adapting tube remaining constantly in the vein, than by thus frequently introducing and withdrawing the nozzle of the syringe. We may mention that we were acquainted with a case in which this method was practised here a few years ago, for introducing a solution of opium into the veins, and inflammation did not ensue. On the contrary, the wound healed rapidly.

IV.

Observations on Tetanus; illustrated by Cases, in which a new and successful Mode of Treatment has been adopted. By HENRY WARD, Surgeon, Gloucester, 1825. pp. 22. 4to.

If the merit of Mr Ward's new and successful remedy is to be judged of by that of the first work in which it is brought before the public, we fear it will enjoy a very short-lived reputation. We shall leave this consideration, however, out of view. The remedy appears, to say the least of it, a rational one; and the importance of the subject makes up so far for the commonness of the treatise, that we should be wanting in duty to our readers, if we did not give them some account of Mr Ward's Observations.

It is rather singular, that a remedy, possessing qualities so peculiar and so obvious as the hydrocyanic acid, should have remained so long in the hands of practitioners without being applied to the treatment of tetanus. The state of the system in tetanus is in many respects so exactly the counterpart of the state induced in most circumstances by the acid, that one would think the disease could hardly be thought of without the remedy suggesting itself simultaneously. Nevertheless it appears to be rightly stated by Mr Ward, that he is the first who has used it, or at least who has published on the subject. How he was led to try it, we cannot conceive. For, if there is any meaning at all in the trite motto he has adorned his title-page with,—bearing, that " Experience is constantly *contradicting* Theory,"—the Prussic acid is the last article of the Materia Medica which could have occurred to him on his Therapeutic principles.

Both of his cases are said to be of the Traumatic variety, one having apparently succeeded a wound of the finger, the other a burn in the arm-pit. His opinion, however, may be questioned, at all events with regard to the former; because, independently of other considerations to be stated presently, the tetanus did not supervene till five months after the injury.

The first case appears to have been introduced by an attack of general fever, accompanied even with some tendency to delirium. The tetanic spasms did not commence till the fifth day of the patient's illness. On that day, however, " a very strong disposition to tetanus was evinced." In what that disposition consisted he does not instruct us. Opium, æther, and camphor, were given without avail. The spasms and delirium having gone on increasing, the hydrocyanic acid was given in conjunction with port wine and brandy. In the course of the first night, 59 drops of the acid were taken, and four bottles of Port and a pint and a half of brandy. The spasms first abated; the delirium continued for some time; but at length the patient fell asleep, and he remained so during most of the day. When he awoke the spasms returned; but through the same treatment they were again mitigated, and he again fell asleep. On his awaking the spasms returned a third time, and a third time they were removed by prussic acid, port and brandy together. From this period he recovered rapidly till the twenty-seventh day after his first attack. Having relapsed on that day, opium was given in the first instance, but without benefit: on the other hand, the spasms immediately yielded to the hydrocyanic acid. On the sixth morning of this new attack, he was much better; " he complained," to use Mr Ward's own phraseology, " of no tendency to a return of spasm;" and, except that he had this singular complaint, and likewise some incoherency, restlessness and local pains, he went on improving steadily, and soon got quite well.

On reviewing the whole particulars of the case, such as they are related,—and that is meagrely enough,—it cannot be denied, that making the man drunk with ratafia'd brandy and port, had the effect of checking tetanoid spasm; and consequently that our author's treatment was both new and successful. But that this was a case of traumatic tetanus, and that it was cured by prussic acid, can never be admitted, unless indeed we make the sweeping assumption, that all this inordinate quantity of port and brandy went for nothing in the cure. The case, seriously speaking, appears to us, in every particular mentioned by Mr Ward, closely allied to an affection that occurred occasionally in this city during the Epidemic Fever of 1818-20, and was considered by us as a spasmodic variety of it. It is not improbable that Mr Ward's practice, bating his brandy diluted with port, would have been of service in that affection; which, we should remark, was so deadly, that, of all the cases we knew, only one recovered.

The second case is more pointed. The affection seems to have been really tetanus, and the effect of the acid was not so much confounded with that of other remedies; for "no quantity of wine could be conveyed into the stomach." The subject of it was a child eight years old, who, ten weeks after sustaining a severe burn in the arm-pit, was attacked with tetanic symptoms. The limbs were extended, the eyes fixed, the belly tense, the jaws locked, the tendons affected with subsultus.

"Having so lately seen a case of this nature, in which the hydrocyanic acid had been so signally beneficial, when administered in large doses, I directed eighteen drops in twelve drachms of cinnamon water, and a drachm of the mixture to be given every half hour, in conjunction with port wine, and the wound to be dressed, &c. The medicine was given for the first three hours regularly, but *no quantity* of wine could be conveyed into the stomach. It, (what?) however, had the most decided effect, as the spasms were greatly relieved, and the child had some sleep, which, however, was very much disturbed. I now directed the same quantity to be given every four hours, in conjunction with the wine. This plan was persisted in until the 16th, (15th of the disease), when the medicine was given only twice a day. Considering these means amply sufficient to quiet the spasmodic tendency, and no appearance of the complaint being present on the 7th, she was directed to take the acid only at bed-time. She now appeared to have overcome the violence of the spasms, although throughout the cure she had occasional attacks, which were invariably relieved by a dose of the acid. The wound has now been healed upwards of two months, since which she has had no return of the complaint."

The few remaining pages of the pamphlet are occupied with some general remarks on the remedy. He first states, that recoveries from traumatic tetanus are rare, because Dr Thomas never saw but one case of it cured; then expresses his suspicions that the acid may act, although it is introduced elsewhere than into the stomach, because a dog was killed by a little of it put on his nose, and he himself lost the use of his limbs for a time, by breathing the vapour issuing from his patient (?); next relates the best process for preparing the medicinal acid; and, finally, prophesies the probable consequences of his labours, and hints at his claims for immortality, by assuring us, in the words of Cicero, that "in nullâ re propius ad Deos homines accedere, quam salutem hominibus dando."

We have already stated our reasons for thinking that the method of cure recommended by Mr Ward is well worth a fair trial. But if he should try it again, or any one else should feel encouraged to follow his example, we may venture to advise him to economize his port and brandy. For, in the first place, it has already been often enough proved, that little or no advantage is gained to tetanic patients, by

"Making their throats a thoroughfare for wine;"

secondly, it is impossible to learn, under such circumstances, which is the useful remedy; and, lastly, although it may turn out that port and brandy are good auxiliaries, it is irrational not to try, in the first instance, the effect of the principal remedy.

We must also advise our author, before he publishes again his conjectures and presumptions regarding the mode of action of hydrocyanic acid, to make sure that he does not guess at things which have been long established by a multitude of facts, and admitted by every well-informed physiologist.

V.

Ueber die Werwindungen des Linsensystems. Von Dr Fried. Christ. Dieterich. Mit einer Vorrede vom Profesor Riecke. Tubingen, 1824. S. 100. 8vo.

On Wounds of the Lens and its Capsule. By Dr F. C. Dieterich, with a Preface from Prof. Riecke. Tubingen. pp. 100. 8vo. 1824.

The length to which the French, in the zealous pursuit of physiology, have carried their experiments on animals, has, from time to time, been the subject of considerable discussion. Whilst some, chiefly extra-professional persons, in their sympathy for the brute creation, have denied the propriety of making such experiments altogether, several celebrated professional characters have given their opinion as to the extent to which they should be carried. The idea of preventing persons from experimenting on animals *in toto*, can only for a moment be entertained by those who are ignorant of the connexion which physiology bears to medicine, and of the true means by which this study can be advanced. The extent, however, to which experiments on animals should, with propriety, be carried, is not easy to be defined, and must, we think, be left, in every case, to the judgment of the experimenter. Views may hereafter suggest themselves to the physiological inquirer of which we can at present have no idea, and, for the investigation of which, it is quite impossible to say to what extent experiments on animals may be justified. But if it be proper to allow the physiologist to use his own discretion in the performance of experiments, it is certainly his duty to accompany them with as little pain as is possibly consistent with the accomplishment of the object which he has in view. The mere putting an animal to death can scarcely be considered an act of cruelty; but the accompanying that act with protracted suffering, can only be justified by the expectation of some considerable benefit from the experiment. As to the experiments before us, they will scarcely be objected to by any one, however fastidious he may be, and were instituted for the purpose of investigating one of the most important points in surgery.

We learn from the Preface, that the book is an Inaugural Dissertation, and, at the same time, the Essay which obtained the prize offered by the Medical Faculty of Tubingen for the year 1821-22. The following is the subject proposed by that learned body. "Cum successiva evolutio cataractæ traumaticæ nondum satis accurate observata sit, diligentius jam exponantur phænomena quæ lentis crystallinæ systemati punctione vulnerato in mammalium oculo de die in diem sese excipiunt. Imprimis disquiratur, *primum* quomodo phenomena modificentur prout aut sola capsula lentis, sive anterior sive posterior, aut etiam ipsa lens vulneratur; *deinde* quodnam momentum vapores acidorum concentratorum et naphtharum ad excitandam cataractam liquoris Morgagni habeant; *denique* num capsulæ lentem afficiens inflammatio et lentis obscuratio incipiens, iteratâ evacuatione humoris aquei tolli queant?

"Omnia, quoad fieri potest, additis delineationibus et præparatis anatomicis illustrentur."

Experiments on the regeneration of wounded parts have of late years formed the subject of several academic dissertations at Tubingen; and we need only mention AUTENRIETH's experiments on wounds of cartilage and veins—those of EMMERTS on the wounds of the œsophagus and the mucous membranes —and of MÜLLER on injuries of the salivary ducts, &c.—to recall to mind the improvements conferred on medicine by these productions. The Essay before us does not fall short in any respect of those which have preceded it; and, in so far as the author has been able to treat the subject, the results of his labours are very apparent. The whole work is divided into five principal parts, of which we will now proceed to give an account.

I. *Wounds of the Anterior Capsule.*—Under this head are described thirty-six experiments, which were performed on the eyes of different animals, particularly cats and dogs. The wound of the anterior capsule was made through the cornea, by means of a straight depression needle; but in no case was it followed by cataract of the capsule. Out of fourteen puncture wounds, there was observed once a white flake of a pyramidal shape on the anterior capsule; once also lenticular cataract ensued, most probably, however, from the lens having been wounded at the same time. In almost every case, a smaller or larger white flake formed itself on the anterior capsule, of which we shall speak more fully hereafter. The punctured parts generally closed together, after the disappearance of this white flake, without leaving any cicatrix behind. When the operation was repeated several times on the same spot, adhesions took place between the edges of the wounded capsule and the healthy lens beneath. The anterior capsule was divided in an horizontal direction nine times, the incision being carried from the inner to the outer edge; and in no case did capsular cataract occur. Once only lenticular cataract followed; but the lens was also here most probably wounded, as the animal was very restless during the performance of the experiment. The wound presented just the same appearance, and healed in the same way as in the other cases. In seven eyes, the anterior capsule was wounded in a perpendicular direction, but these wounds also had no influence on the transparency of the capsule. The wounds made in this direction generally gaped more, and healed more slowly than the others. In one case, where a transverse and perpendicular wound of the capsule had been made forty days prior to the death of the animal, there was found, on examination, an opacity proceeding from the centre to the periphery, situated between the anterior capsule and the lens, which consisted of fine white lines, connected both with the lens and the capsule. Nearly similar cataracts have been observed by MORAND and HOIN. MORAND [*] found, in a lenticular cataract, that a thready net extended itself completely from the middle of the lens to its circumference. HOIN [†] mentions a case of similar cataract; the patient who had it could still distinguish colours and larger objects. She died in the hospital; and on examining the eye after death, this surgeon found the following appearances. "The lens was a little harder than usual, and in part opaque; there were several white striæ in the centre; these extended towards the outer surface of the crystalline, and penetrated through the whole substance of the lens." Neither of these two authors make any mention of the original production of this cataract.

Six experiments were instituted, in order to know what influence incisions in the anterior capsule, in different directions at the same time, might have on the production of capsular cataract; but in one case only was there observed a small white flake on the anterior capsule, which was formed of fine white threads, running parallel to one another. Lenticular cataract also occurred once, but certainly from disturbance of the lens at the time of the experiment, because the anterior capsule was so delicate that, on dividing it, the lens must necessarily have been pressed and moved from its situation, and because the cataract occurred quickly after the wound.

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[*] Mercure de France, M. Aug. 1759.

[†] Memoires de l'Academie de Dijon, tom. i. 1769.

From the incisions in different directions, flaps were formed in the capsule, which were generally absorbed, so that a large round opening usually remained behind, the edges of which were surrounded with a whitish flake, and this again was connected to the lens.

From these facts Dr Dieterich draws the following conclusions; 1st, That the anterior capsule contributes very little to the nutrition of the lens, as the greatest disturbance to it, with the exception of actual laceration, had no influence on the lens. MOHRENHEIM relates a case * which proves the truth of this as far as relates to the human eye, in a still higher degree; "being aware," says he, "that the patient had capsular cataract, I pulled the capsule with a small hook to the bottom of the anterior chamber, and allowed it to remain there, because its inferior edge was still firmly attached. As the patient now saw, I allowed the lens to remain, and the person always retained tolerable sight. Also PELLIER † operated in a similar way on a patient in whom the anterior capsule only was opaque, and after drawing this out the patient saw, on which account the lens was allowed to remain. 2d, That the anterior capsule has a great power of preserving its vitality, as, in most of the cases, even in very large wounds, it did not become opaque; and simple, particularly transverse wounds, healed completely, and even without any cicatrix. Thus RICHTER ‡ says, that among fifty cataract patients he only found one in whom the anterior capsule was opaque, and HENKEL § found, on the examination of a person on whom the operation of depression had been performed in his lifetime, (how long before death is unfortunately not said), the whole capsule quite transparent; the wound of the anterior capsule was perfectly healed; through the posterior capsule only there was an open-

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* Richter's Bibliothek, B. vi. St. 4. p. 582.

† Richter's Bibliothek, B. viii. St. 1. p. 29.

‡ Bibl. B. ii. St. 2. p. 2.

§ Richter's Bib. B. v. St. 3. p. 422.

ing into the *corpus vitreum*, just at that part through which the lens had been pushed. The cavity within the capsule was filled with a fluid. We may mention here a case which **Richter**, in the second volume of the Bibliothek, (p. 226), has related, where, in an old man, the posterior capsule was opaque, and in some places ossified, and adhering to the hyaloid membrane, whilst the anterior capsule was quite transparent. This also proves that the two divisions of the capsule have a different life or organization; they are, moreover, of a different structure, since the anterior is well known to be thicker than the posterior capsule.

With respect to the origin of the white flake which we noticed above, as being observed on the surface of the anterior capsule, Dr **Dieterich** says there are only three ways in which it can be explained; 1st, it arose either from the humor Morgagni; or 2d, from the external semifluid layer of the lens; or, 3d, it was formed by the secretion of a particular kind of matter, perhaps of plastic lymph. Our author is inclined to adopt the first of these opinions, but confesses, that to arrive at any certainty about it, a careful examination of the chemical properties of the humor Morgagni, for the purpose of ascertaining whether it contains the requisite properties, is necessary. It appears that **Reil** thought that it did, since he calls it a coagulable lymph. The office which the flake performed was that of mechanically closing the wound of the capsule, and preventing the exit of the aqueous humour from the lens.

II. *Wounds of the posterior Capsule.*—Cataract of the posterior capsule was produced with still greater difficulty than that of the anterior. From all the experiments made on the posterior capsule, it was found to be as little irritable as the anterior; and although it most probably receives branches from the *arteria centralis retinæ*, and several from the arteries of the ciliary body, still Dr Dieterich never saw a trace of inflammation or vessel in it. There was one point observed in these experiments which may be worth relating, that lenticular cataract was more frequently produced by wounds of the posterior capsule, than in the preceding experiments; from which circumstance the author draws the conclusion, that it has more to do with the nourishment of the lens than the anterior capsule has. We are, however, inclined to suspect that the cause of the more frequent occurrence of lenticular cataract may be explained, by the greater difficulty of performing these experiments without injuring the lens. In seven experiments of simple puncture of the posterior capsule, two were unattended with any change in the eye. In three cases there was visible far back a haziness, (probably the usual flake which, from its depth in the eye, could not be accurately distinguished), which, however, disappeared in 21 days. Once lenticular cataract followed, and in another case iritis and suppuration of nearly the whole of the globe. The posterior capsule was cut into in twelve cases, in eleven of which lenticular cataract followed; in five the cataract developed itself without any sign of inflammation in the eye; in three there were some slight symptoms of iritis, and in three cases it began with obscurity of the vitreous humour. The puncture wounds merely healed; the incised wounds were never found closed.

III. *Wounds of the Lens.*—The experiments which were performed on the lens may be classed under three heads. 1. Wounds of the anterior, or posterior surface; 2. Those of the body of the lens; and 3. the removal of the lens from its natural situation. From all these experiments it was found that the lens possesses a very slight degree of irritability; that its surfaces are less irritable than the centre; that wounds of the anterior surface produced cataract much less frequently than those of the posterior; that the soft lens of young animals could bear the greatest wounds without any injury, but that displacement was always attended with the death of the lens, the capsule, however, always remaining clear. The wounds of the anterior surface of the lens produced in no case any apparent influence either on the lens itself, or the capsule, or any part of the eye. In seventeen puncture wounds of the lens, twelve were not attended with any change in the eye, with the exception of the usual white flake; in one case iritis occurred, without producing any mischief to the eye, and in the other suppuration of the globe; in three cases lenticular cataract was produced, two being attended with iritis, and the third not. In nine cases of simple incised wounds of the lens from its anterior surface, four were unattended with any pathological change in the eye; but in these the wound was carried from the edge to the centre, and not quite through the substance of the lens. In five other cases, on the contrary, where the lens was nearly divided by the wound, lenticular cataract followed each time. The lens was divided in eight cases in different directions at the same time; and when the incisions did not penetrate beyond the anterior half of the lens, they were not followed by any change in the part. In one case a piece was, in the performance of the experiment, cut out of the lens. This became opaque, and was absorbed, whilst the remainder of the lens retained its transparency. In another case, the divided lens was nearly altogether absorbed, without any apparent change being first produced in it, and the remaining capsule was left nearly in its natural state. A remarkable appearance here was the haziness of the internal coat of the cornea, the result no doubt of inflammation of the *membrana humoris aquei*. In five cases of wounds of the lens from its posterior surface, two were followed by no change whatever; but in these the wound penetrated a very slight way into the lens. In one case the eye was destroyed by the inflammation which supervened, and in the other two, lenticular cataract was produced. In eleven experiments of displacement of the lens, three only were unattended with lenticular cataract; in the other cases the lens became opaque, and at the same time violent inflammation of the iris and the ciliary bodies generally occurred.

Superficial wounds of the lens unaccompanied with any disturbance from its natural situation, or injuries of other parts, were followed by no change whatever in the eye, even when they penetrated a quarter of the diameter of the lens; the wounds were, in a few days after the experiments, completely healed. That the wounds of the posterior surface of the lens were more frequently followed by lenticular cataract and inflammation of the eye, than those of the anterior, is, (and we think very properly so,) attributed by our author, not to any difference in the structure of the two surfaces, but principally to the great difficulty which exists in reaching the posterior surface, without injuring several other important parts, and displacing the lens. The deeper wounds, particularly those which reached the centre of the lens, were in all the animals followed by lenticular cataract, iritis, inflammation of the globe itself, effects which were probably to be attributed more to the concussion and displacement of the capsule and neighbouring parts, than the wounds of the lens. But in what manner inflammation of these parts produced cataract, Dr DIETERICH confesses he is at a loss to explain, since he was never able to discover in cases where the surrounding parts were streaming with blood, a single blood-vessel in the capsule or lens.

IV. *Application of the Concentrated Acids and the Æthers, both as liquids direct to the eye, and in the form of vapour.*—The sulphuric, muriatic, and nitric acids produced nearly the same effects on the eye. Their vapours merely caused a superficial inflammation of the cornea, and sometimes also of the conjunctiva. The direct application of these acids to the cornea, sometimes produced speedy destruction of its superficial layers, and at others penetrated it. The inflammation in the internal part of the eye was never very severe, seldom did any extravasation take place, and still more rarely did the eye go into a state of suppuration. When the sore did not quite penetrate the cornea, the cure rapidly followed, and the cicatrix was in general transparent. When, however, the cornea was eaten through by the acid, a circle of vessels formed themselves round the part, and the loss of substance was repaired by a body or coat, which was in some degree transparent. The phosphoric acid had a much slighter influence on the eye than the other three, and the acetic acid the slightest. The ethers produced scarcely any effect at all. The eyes of young animals were more affected by these acids than those of adult animals, which is the contrary with what occurred in the wounds of the lens. Cataract of the *humor Morgagni* was observed in no case.

V. *Evacuation of the Aqueous Humour in Incipient Lenticular Cataract.*—When the opacity of the lens was considerably advanced, it was never removed by the evacuation of the aqueous humour; at most its complete formation was only retarded. If, on the contrary, the opacity was still inconsiderable, the repeated evacuation of the humour had the effect of entirely removing it; for, on killing the animals, the lens was always found transparent. But here it may be doubted whether the opacity was really in the lens or not, or, allowing it to have been situated there, whether the disappearance of the opacity would not have taken place without the evacuation of the humour. A very important point was ascertained in these experiments, that if inflammation of the eye, particularly of the iris, existed at the time the aqueous humour was evacuated, the disease became worse, and suppuration of the globe followed. In every case where the capsule was properly opened, and the inflammation of the eye was not too violent, the absorption of the lens was always promoted by the evacuation of the aqueous humour. From this circumstance Dr DIETERICH draws the following conclusions. 1. He thinks, that, in incipient cataract, it is very probable that the formation of the complaint may be prevented, or the absorption of the lens produced, by the evacuation of the aqueous humour. 2. That this mode of treatment is particularly applicable, after those operations for cataract, in which portions of the lens may remain unabsorbed. On the first of these points it is impossible to give any decisive opinion, as it can alone be determined by actual trial on the human subject; but as to the second, we see no advantage which is to be obtained from the evacuation of the aqueous humour over a repetition of the operation with the needle. The last is more certain, and assuredly not more painful than the former. The evacuation of the humour is always contraindicated when inflammation of any part of the eye exists. Dr Dieterich gives the following cautions in the letting out of the aqueous humour, which may be useful to those who repeat the experiments. 1*st*, The cornea is not to be punctured with a needle, but with a sharp lancet, or a good cataract knife, because the puncture made with these instruments remains longer open, and a repetition of the operation is rendered unnecessary. 2*d*, The opening must not be made in the inferior part, in order to prevent the whole of the aqueous humour escaping at one time, because, in that case, *prolapsus iridis*, or adhesion of the iris to the cornea, might easily take place. The puncture should be made as nearly as possible to the outer edge of the cornea, in a perpendicular direction, and a line in length. If the puncture requires to be repeated, it must be made at some distance from the first, and never attempted before the other is healed.

These are the results of Dr Dieterich's experiments; and, in so far as the author has investigated the subject, they are highly important. It would have been very desirable if he had extended his observations to the reproduction of the lens, for the purpose of ascertaining if, in any of the experiments, the removed parts of the lens were regenerated. The book is, on the whole, a valuable production for the information which it contains on the effects of wounds of the eye, and the reparation of wounded parts; and we only wish that the author may continue his future observations on these points with the same care and minuteness as he has done the past.

VI.

Collections from the unpublished Medical Writings of the late Caleb Hillier Parry, M.D. F.R.S., &c. &c. Vol. I. London, Underwoods, Fleet-Street, 1825.

There is something highly flattering to the pride of British intellect in the history of medical theory and practice, during the last thirty or forty years. The changes which have taken place in opinion and principle are so much more important, and acquire so much higher interest, that they have led to modes of practice greatly more simple and efficacious, than those which were in the hands of our predecessors. That the practice of medicine is much more successful, or that the number of curable diseases is increased, and the mortality of the human race is greatly diminished, we do not pretend to assert. But it is undeniable that, of many individual diseases, and of several tribes of disorders, the treatment is at present much more efficacious than it formerly was; and if the number of disorders incident to the human frame is not diminished, not only is the cure of several rendered greatly more certain, and greatly more expeditious than according to the method of the older physicians,—but many maladies, which were invariably stamped with fatal characters in the books of the last century, are found in the present day to be occasionally amenable to the art of healing, or at least to disappear under the use of appropriate management.

Thus though fevers, inflammations and apoplexies, continue to kill a considerable proportion of the human race, it is well known that neither fevers, inflammations, nor apoplexies are necessarily fatal; and that a great proportion of them are brought to a favourable termination by the prompt and energetic use of a few well chosen remedies. Palsy still continues to numb the limbs, and dropsy to swell the person of its victim; but neither palsy nor dropsy are so frequently or invariably fatal, as they have been hitherto represented to be; and much, we know, may be done to prevent a bad termination, by the seasonable employment of powerful and well-ordered measures. That the practice of medicine, in short, has undergone essential improvement is so obvious, that it would be equally idle to maintain the point, or to suppose it necessary to do so.

But when we come to inquire in what the improvement consists, we suggest for the consideration of the practitioner, a subject not less useful in the conclusions to which it leads, than in the extensive views which it lays open. If a closer and more sedulous observation of disease, if a more diligent and extended cultivation of pathological anatomy have thrown open to the eyes of the physician varieties in the endless ramifications of morbid action of which he was formerly ignorant, they have also enabled him to generalize on a more solid basis, and to perceive more clearly and more certainly than before, the great general facts to which all these varieties are ultimately referable. If the great multiplicity of effects resulting from the operation of the same cause, has been capable of perplexing his previous ideas on the uniformity and simplicity of its operation, he has thereby been able to discover more clearly the circumstances which modified, and the means which might counteract these effects. If more correct experience and more careful examination has shown effects of morbid action, against which his best and most powerful means must have been necessarily unavailing, he has also learnt, by watching the approaches and earliest symptoms of unhealthy action, that there is a period when a chance of success is not uncertain, and when incipient disease may be strangled in the birth. And if a sounder theory has taught him to take a new and more correct view of the essential or pathological cause of a disease, without confounding it, as has been too often done, with mere symptoms, he has also learnt to abandon the practice of prescribing for symptoms only, and to rest his hopes of successful treatment on the employment of a few powerful and well-chosen agents, which exercise an undoubted influence on the functions of the living body, and on the morbid process under which it is labouring. Such views constitute the true philosophy of medicine.

Among the authors whose writings have mainly contributed to the effects to which we now allude, few can be said to hold a more distinguished rank than the late Dr Parry. Sedulous observation, accurate research, with considerable originality of thought, were the characteristic features of his mind. The boldness of his conjectures and the freedom with which he speculated on the nature of diseases, would have been extremely pernicious, if his observation had not been accurate, and his judgment correct. But while he was on the one hand cautious to guard against false or erroneously reported facts, he was on the other studious, that his reasoning should be conclusive, and that his inductions should rest on a solid foundation.

With several of the views of this physician, our readers are already partially acquainted by means of his Elements of Pathology and Therapeutics. But it is partially only, for this work is manifestly the mere introduction, or preliminary part of a system which the author undoubtedly intended to publish in a more perfect and comprehensive form. It has been indeed said that this work was mis-named by the ingenious author; for instead of being Elements of Pathology, it turned out to be Elements of Physiology. The remark is not altogether unjust; but it must be remembered, that to understand clearly the ideas of Dr Parry on the disorders of the human frame, it was in some sort requisite to be aware of the peculiar views which he took of it in its healthy condition; and this was manifestly his object in the first part already published.

Dr Parry lived not to accomplish the great design which he had conceived. That he had to the last entertained hopes of being able to effect his purpose, and had with this view been proceeding diligently and steadily amidst the fatigues of his professional engagements in collecting, digesting and improving the materials, is manifest from the state in which his papers had been left. But assiduous and indefatigable as he was, it was in hope only that he was suffered to indulge. Long ere his collections had attained that perfect form in which he deemed them worthy of the public eye, the faculties of Parry were veiled in a fatal lethargy; and one of its strongest intellects and most judicious speculators was snatched for ever from the profession of medicine.

In this case one of two courses only could be followed; either to suppress entirely the results of the experience and reflection of a physician already high in scientific estimation, lest the desultary form and imperfect state of his reasonings should impair the character which his former writings had acquired;—or to lay a selection of them before the public in as finished a shape as their situation admitted, in the hope that the matter would be sufficiently important to render them not unworthy of the name of the author. Each plan had its difficulties, and it must have required no ordinary deliberation and discretion to balance them so accurately as to form a sound decision. By withholding the whole, it is not unlikely that the healing art might have suffered a greater injury, than all the benefit, which could have accrued to the reputation of the author, could compensate. On the other hand it is not even every medical work, the matter of which will compensate for defects of manner; nor will every reader be willing to accept of a few valuable facts and novel conclusions amidst a great mass of desultory, vague, or commonplace observations. It was manifestly the great object to make such a selection as would neither weary by the triteness, nor mislead by the inaccuracy of its views;—to publish a work in which clearness of arrangement, novelty of principle, and soundness of doctrine, should at once combine to please and to instruct.

That this object has been attained in the volume before us, we do not think it will be possible to persuade our readers; but we are satisfied that it has been held in view; and the only apology which we believe can be offered for the failure of its accomplishment, must be sought for in the circumstances to which we have already alluded, and which rendered it in a great measure impracticable. In this light it is requisite to view the present work, in order to make a proper allowance for its defects, as well as to form a just estimate of its merits. Of the former, for from defects no work is entirely free, various opinions will be entertained according to the importance which different readers attach to the peculiar views of Dr Parry, and the manner in which he attempted to unfold and illustrate them. The minuteness of his details, and the superfluous abundance of his proofs and illustrations, may be tiresome to some who are willing to adopt new opinions on the single authority of their promulgator; and the slowness of progress with the number of steps which the learned author appears to have taken to arrive at his conclusions, may seem tedious to those who are unwilling to trace the links, or unable to comprehend the connection and bearing, of a long and complicated process of reasoning. Some of his propositions may appear so obvious as to fall under the head of truisms; others may present a character so uncommon as to merit the imputation of wild or groundless innovations. To us it appears, that a want of distinctness of arrangement, and of due subdivision, are the chief objections which can reasonably be urged against the merits of the work; and as this could never be said to be any fault of the writings of Parry, published by himself, we have no difficulty in acquitting the departed author of any share in this effect. After this general notice of any imperfections, we proceed to speak more fully of the better and more valuable qualities of this publication.

The present is the first volume of a work which is intended to contain collections on the phenomena and effects of diseases observed and treated by Dr Parry during a long and extensive course of professional exertion. He appears to have been early impressed with the importance of the task of accurate observation, and the difficulties with which it was beset in forming general conclusions applicable to practice. In his Introduction, which is written in a clear and philosophical manner, he justly observes, that medical histories and discussions are so uninteresting, that those who begin the practice are confounded by the unexpected complication of phenomena which occurs in nature, and that he who peruses these records with the hope of obtaining instruction, usually suffers mortification and disappointment. These evils Dr Parry ascribes principally to three causes; first, the practice which many observers follow of giving mere results without any detail of the facts on which they are founded; secondly, the habit of relating those particulars only of medical or pathological facts which serve to support the peculiar views of the author; and, thirdly, the want of leisure and inclination which too often is the immediate consequence of that incessant fatigue in the practice of the art, which necessarily attends increase of the opportunities of collecting knowledge. "If to this rule" says Dr Parry "there occur some few exceptions, they depend not on any superiority of original talent, but on early habits of mental application; on the force of motives, on the felicity of local situation, and on the capacity of the body to endure privation and labour, without suffering that languor which would impair the energy of the mind."

It is chiefly to the early acquisition of those habits of diligence, influenced by an ardent desire to distinguish himself by adding something not entirely worthless to the stock of medical and pathological knowledge, that Dr Parry, with equal modesty and sincerity, ascribes any thing he has been able to effect. It may serve to give our readers some idea of the labours of this diligent and original inquirer, and of the confidence which may be reposed in his investigations, if they read this passage in his own words.

"Feelings like these, which have long made my professional pursuits my greatest pleasure, aided by the wish of emulating some great professional names, and by a strong desire, that the world may be the better for me after I shall have left it for ever, have supported me under the privation of domestic and social gratifications, and under exertions incessantly pursued through sickness, sorrow, and pain.

"Whatever judgment the reader may ultimately form of my doctrines, no part of the more recent pathological principles delivered in my various writings, will either be borrowed from others where I may happen to agree with them, and none will be promulgated in order to controvert them, where I differ. As circumstances may suggest, I shall doubtless avail myself of medical books; in them looking not for opinions, but facts; if it may be possible to distinguish the true and the luminous, from that which confounds by fiction, by defective or erroneous observation, or by the substitution of hypothesis for phenomena.

"The facts or opinions which may be derived from others, I shall scrupulously attribute to their true source. The reader will thus have no difficulty in distinguishing that which is the result of my own investigation from that which is common property. If similar observations or deductions are to be found in books which have not been consulted, I shall consider this as a happy coincidence, confirming the truth of my own principles.

"The great Book of Nature which is alike open to all, and is incapable of deceiving, I have hourly read, and I trust not wholly in vain. During the first twelve or fourteen years of my professional life, I recorded almost every case which occurred to me, either in private practice, or in the chief conduct of an extensive charity. When afterwards the multiplication of common examples seemed to me an unnecessary waste of inestimable time, which might be much more profitably employed, I contented myself with the more useful task of recording chiefly such cases, or on a few occasions such particular circumstances only of cases, as led to the establishment of principles. This I have generally done on the spot, or rarely deferred beyond the day of observation; always rejecting what, on repeated or varied inquiry, I have not been able to verify.

"Whatever inferences from phenomena have suggested themselves to me, I have immediately noted down, and afterwards carefully examined on all sides, and in every light. By this method I have been able to ascertain the order of phenomena, and to catch new links, which have gone some way towards completing the whole chain of causes and effects."

By the diligent pursuit of such plans, Dr Parry was enabled to collect numerous examples of morbid action, of the circumstances under which they proved fatal, and of the means by which they were not unfrequently counteracted. From the materials thus collected, comprehending a considerable number of dissections, and nearly seven hundred illustrative cases, it was the intention of the author to compose a careful inquiry into the physiology and pathology of the human frame, and into the most effectual means of preventing, alleviating and curing certain diseases. This was not, however, designed to be a complete history of all diseases; and Dr Parry intended to confine his researches to the consideration of those only which had fallen under his own observation. Some of them indeed, are the most important that can affect the human body; and one of his great objects was to show, that many disorders which, not only according to popular opinion, but in the estimation of professional persons, are of opposite nature, do at certain periods of their course agree in one common circumstance, and that their theory may be simplified, and an important and beneficial change may be effected in their mode of treatment.

The first part of the work consists of a short view of General Anatomy and Physiology, in which the following subjects are slightly touched;—Anatomical systems by Haller, Soemmering, and Bichat; Circulating system, the operation of nervous influence, tonicity and irritability on it; topical action of vessels; original distribution and changes of arteries; tortuosity of arteries, the capillary system, dilatability and contractility of the veins; the influence of the heart on the venous circulation; the ideas of Bichat on the blood, as consisting of a recremental and excremental portion, and its coagulation; animal heat; relation of the motions of respiration and circulation; effect of the muscles of the epiglottis; on the phenomena, causes and varieties of muscular action, both in the system of voluntary and involuntary muscles; the influence of the nervous system; and some observations on the hair and skin. The inquiry is manifestly imperfect and desultory, and we pass from it to more attractive matter.

One of the favourite subjects of speculation with Dr Parry was that, to which he and many others have given the name of irregular determination of blood. What is the precise nature of this irregular determination, or what is the state of the vascular system during such irregularity, has never been very clearly or accurately defined;—probably because it was believed to be so obvious and intelligible, as to render explanation superfluous; and it is rather with the supposed causes of such irregularities, than with its essential nature, that the inquiries of physicians have been occupied. "In irritable habits" says Dr Parry, "there is a disposition to irregular determinations of blood. It is true that *these determinations must have a cause*; but the question is, whether the effects which we witness, viz. the symptoms of disease, would ever exist, if there were not this intermediate and *secondary state of vascular fulness*, though a cause producing this state may really exist." There is here a want of clear conception and of precise language, which is manifestly to be ascribed to the neglect of previously defining what is meant by irregular determination. The mode of expression above used would lead to the supposition, that, though the author entertained no doubt of the existence of the remote causes, and of the mere symptoms, and of the necessary connection between these two orders of phenomena,—yet he was very uncertain about that state of the vessels on which the external symptoms depended, and which constitutes the morbid process or pathological essence of the disease. He calls it indeed a *state of vascular fulness*, but he regards it as secondary only; and he makes no attempt to show how this vascular fulness arises, which he admits is a proof of irregular determination.

It subsequently appears, that irregular determination consists in the destruction or change of the balance of circulation, which takes place in two modes; either by diminishing the flow of blood to one part, and so increasing it to another,—or by weakening the propelling power in the vessels of a part, and so producing accumulations in it. This view lays open a wide field of hypothesis and conjecture; and as it forms the basis of many of the reasonings of Dr Parry, it may be worth while to examine the nature of the grounds on which it rests, and to inquire into the certainty of that evidence on which the idea of irregular determination is supported.

In the natural and healthy state of the circulation, it is well known that the blood which flows through the arteries into their minute divisions in the substance of the various organs, passes into the small veins, and successively into the larger ones, in its return to the heart, so uniformly, so regularly and so speedily, that it is not suffered to make any long stay in any set of capillaries, or in the small vessels of any part of the human body. This fact may be said to be so thoroughly established by the accurate experiments of HALLER, but above all by the varied and extensive ones of SPALLANZANI, as to render further proof quite unnecessary. At the same time, while this easy and fluent state of the circulation continues, all the processes which depend on the circulation are performed with accuracy and facility. Exhalation, whether in the serous, the mucous, or the synovial membranes, secretion, whether directly from arteries, or through the medium of glands, and, above all, the molecular deposition of nutrition, are all performed with that regularity and efficiency which constitutes what is termed health. Thus far we may advance with tolerable security; but beyond this there is much uncertainty and conjecture.

Observation, however, of the living body, and examination of the dead, with sundry and numerous experiments on the lower animals, inform us that the blood-vessels of any given part of the animal body may become unusually distended, and unusually numerous. JOHN HUNTER was in the habit of saying that new vessels might be, and actually were formed; and BICHAT has, to a certain extent, admitted this to be the case. It is however quite uncertain, whether these are actually new vessels, or colourless capillaries only distended with red blood,—an opinion which, though a key-stone in the Boerhaavian arch of *error loci*, we cannot fail to regard as infinitely more probable than the formation of new vessels. That the blood-vessels are thus liable to occasional extreme distension, is so well established by the phenomena of many diseases, that it would be captious to deny it, and idle to adduce proofs in its defence. There are other points, however, which are problematical, and on which precise facts are still wanting. These may be referred chiefly to three heads. First, what is the pathological nature of this capillary distension, and how is it produced? Is this distension of capillaries invariably connected with a definite order of symptoms? Are the symptoms alleged to arise from this capillary distension in all instanees, and without exception, to be traced to it, and to no other cause?

On the first of these subjects, on which alone we propose at present to make a few observations, it is remarkable, that although it forms the great basis of the pathological views of Dr Parry, yet he does not seem to have bestowed so much research as perhaps might be expected. His first object is to determine the proximate cause of inflammation. "The first phenomena of inflammation," says he, "are always increased *momentum*, that is, increased velocity with the same quantity, increased velocity with increased quantity, equal velocity with increased quantity, or lastly, diminished velocity with increased quantity; but as local inflammation often takes place without increased action of the heart, it should seem, that in such cases, the two last states only can exist; unless we suppose a fact, which has never been proved, that certain arteries, more remote than the part evidently affected with excessive fulness, act as a heart with regard to that part." From this Dr Parry is inclined to conclude, that there must be some state of the vessels of the part which may give occasion to the increased local fulness that constitutes the increased *momentum;* and he further infers, that, if this state of the vessels act as the cause of the topical increased *momentum*, it must necessarily be more remote than the *momentum* itself, and therefore cannot be considered as the proximate cause.

Now the very outset of this reasoning is beset with a condition, which we cannot help regarding as a vulgar error arising from superficial observation only. Dr Parry sets out with the assumption, that the first phenomenon of inflammation is increased *momentum*, which he explains to be increased velocity with the same, or a greater quantity of the propelled fluid. A little reflection, however, will soon show, that there is scarcely a single instance either of inflammation or of irregular determination, in which the velocity of the blood is truly increased. The heart indeed contracts more frequently in a given time, and more speedily than it does in the healthy state of the circulation; and this appears to have led to the opinion, that it propels the blood more rapidly in a given time than natural. But nothing can be more fallacious or less consistent with fact. Numerous experiments and observations show, that the blood moves most easily and rapidly, when the contractions of the heart are not more frequent than natural, and when these contractions are performed so slowly and regularly as to ensure the effectual evacuation of both ventricles. Wherever these contractions become more frequent in a given time than usual, or are performed more quickly than they are wont, it may be justly inferred, that the blood is propelled very ineffectually through the great arteries, and is moving much more slowly than natural through the small ones and their capillary terminations.

It therefore appears, that the idea which Dr Parry, in common with many other physiologists, entertained regarding the increase of velocity in the motion of the blood, is a gratuitous assumption only; and that, whether the quantity of blood in an inflamed, congested, or otherwise diseased part, be increased or remain the same, the freedom of motion is impaired, and in short the natural velocity with which the blood moves through the vessels of that part is diminished. It is further manifest, that if, by any means, this natural velocity be diminished, and continue so, the quantity of blood in the vessels of the part, which at first might not be very great, will be gradually but steadily increased, until it comes to be very considerable; and this would appear to be the true cause of the great accumulation of blood in any part, which is in the state of inflammation or congestion, and of the unusual distension of its vessels.

Having thus disposed of the idea of increased velocity in the motion of the blood, we shall perhaps see a little more distinctly than before, what is to be done with that of increased *momentum*. We are not certain that Dr Parry uses this term in the proper sense. It was originally borrowed by the mechanical and mathematical physicians from the science of mechanics; and it was used to denote the *force* with which the blood is propelled. Dr Parry appears to think that it is either the same with the velocity of motion, or that it implies velocity of motion;—a mistake which will be readily rectified by any one who attends to the force with which the heart contracts in its healthy state, in contradistinction to that which it occasionally exercises, when it has an obstacle to overcome. In the one case its contraction is steady, forcible, and at the same time perfectly efficient; in the other it is forcible, jerking, labouring, and inefficient. The heart of a healthy adult contracts with true, natural force, and with the effect of perfectly emptying the cavities; but let the semilunar aortic valves of the same adult be ossified, enlarged, or otherwise diseased, and the same heart will be found to contract forcibly indeed, perhaps with a greater degree of force than formerly, but with a labouring and ineffective struggle, as if utterly unable to overcome the resistance opposed to it. In this case, it cannot be doubted, that though the force of the heart is greater, the *momentum* of the blood is actually diminished; and if no other proof than this were at hand, the languid and feeble impulse, with which the blood strikes against the arterial tube would be perfectly satisfactory. The arterial pulse under such circumstances is always weak. It would be paradoxical, however, to maintain, that, because the heart in the latter can contract with greater force than in the former, it was in a more healthy state. It is indeed roused to an unusual and unnatural effort, which is equally injurious to its own powers, and inefficient to the proper performance of the function for which it is designed.

This appears to be precisely the state of the doctrine of increased *momentum*. Because the heart, in many diseases, contracts with great force, and actually transmits the shock or impulse of this unnatural and jerking motion to the arteries, it is therefore concluded, that the blood moves with greater force than in the natural state. We are not acquainted with a single fact which is competent to demonstrate that the blood is ever, in such circumstances, propelled with greater force than usual. In point of fact, we believe it would be found to move much less forcibly; and that the whole of the *momentum* which was thus believed to be communicated to it by the heart, would be found to be nothing but the result of the unnatural and violent, but ineffectual efforts to which the heart was roused, when stimulated beyond its powers. The *momentum* of the blood is greatest in the state of health, and impaired or diminished in various degrees in the state of disease.

But Dr Parry assumes, not only a *momentum* of the blood as derived from the heart, but a *momentum* resulting from the action of arteries. At least he admits, what is equivalent, an increased *momentum* in the capillaries, which he represents as preceding the yielding state of certain vessels (those beyond them, we imagine), which allows them to be affected with the excessive fulness that characterizes inflammation, (p. 146.) We know of no fact, or order of facts, which could be adduced in favour of this opinion, which is perhaps less tenable than the one relating to *momentum*, supposed to be derived from the heart. It has been indeed commonly said, that the violent throbbing of the arteries of an inflamed part, the beating of the carotid and temporal arteries in affections of the head, and the beating of the radial artery in instances of whitloe and other inflammations of the hand and fingers, proclaim the unusual force with which the blood is propelled towards these parts respectively. But if these several instances be properly examined, it will be found, that the conclusions regarding the increased action or *momentum* are liable to sundry fallacies.

Admitting that the first of these facts regarding the violent throbbing of the vessels of inflamed parts, was fully and invariably established, it would not follow that the *momentum* of the blood flowing through them was increased. That the action of throbbing is derived originally from the heart, and depends on the shock communicated along the arterial tubes, is manifest from the fact, that the beats are synchronous with the beats of the heart. But that it also depends on the state of the minute vessels of the part, is further to be inferred from the fact, that it occurs not in the arterial system at large, but is confined to these arteries exclusively; and the explanation of the reason why it is particularly found there, is to be sought for, we conceive, in the curious fact to which Dr Parry first called the attention of physiologists, that the sensation of the beat of an artery is distinctly felt only when something is interposed between the arterial tube and the finger. In the throbbing arteries of an inflamed part, not only is this circumstance present by the quantity of fluid necessarily deposited round the minute vessels; but its influence becomes so much more powerful, by the distended state of the vessels, and the tension and unyielding nature of the skin, and other textures concerned. The throbbing, therefore, of an inflamed part, instead of proving increased force in the motion of the blood, indicates only the unusual resistance which the blood encounters in passing through the arteries, and the confined tense condition of these vessels when unusually distended with blood.

If the carotid and temporal arteries are observed to beat more violently than natural in certain affections of the head, this is to be ascribed, not to any increase of force with which the blood is conveyed to the brain, but to the increase which the action of the heart undergoes, as an effect of disorder of the circulating system in general, rendered more conspicuous, as it must be, by the disturbance which takes place in the circulation of the brain. And to this last cause also is to be ascribed the strong pulsation of the radial artery during inflammation of the hand or fingers.

From the facts and arguments now adduced, we conceive it plainly follows, that there is no proof, either that the velocity with which the blood moves, or the *momentum* with which it is propelled, is increased in those diseases in which there is either inflammation or congestion; and that, whatever may be the cause of morbid or irregular determination, neither of these conditions, separately or conjointly, forms any part of the process. It is peculiarly unfortunate that these defects make an important part of the theory of Dr Parry on the nature of a great proportion of diseases; for we admit that the application of the theory is in many respects extremely useful, and to all appearance highly consistent with the phenomena.

"It is highly probable," says Dr. Parry, "first, that in a state of local inflammation there is a morbid determination of blood to the capillary vessels of the part affected, *which consists in an increased momentum in those vessels* which carried blood in the healthy state, or in the intromission of blood where they were previously void of it; but, secondly, that, in a more advanced stage of the disease, a slower circulation and a diminished impetus, or even a total stagnation of that fluid, may ensue; and, thirdly, that there will follow the various effects already specified, chiefly intended either for the cure or the renovation of the affected part."

The increased *momentum*, which we have already attempted to show is a hypothetical or assumed circumstance in this train of events, Dr Parry was disposed to regard as the proximate cause. But the reasons which he assigns for this conclusion are, as may be conjectured, neither very intelligible, nor, so far as we understand them, very satisfactory. It must not, however, be imagined that this difficulty necessarily invalidates the truth of the theory of morbid or irregular determination, which is, in some respects, to be regarded as the mere mode of expressing a fact, of the reality of which physiologists and pathologists are pretty well agreed. The difficulty consists in showing why this morbid determination takes place, and in pointing out the circumstances which dispose a certain set or order of capillaries to be filled and distended to a greater degree than natural, and than the other capillaries of the system at large. Since this distinction began to be perceived, various explanations have been offered by John Hunter, by Bichat and some of his followers, by Wilson Philip, by Thomson, by Hastings, and others. Of the views given by each of these authors, this is not the place to speak; and, as Dr Parry appears to have regarded that of Bichat only as requiring animadversion, our strictures shall be confined to his remarks on that only.

Dr Parry, we have seen, assumes the circumstance of an afflux of blood to a certain part; and incontinently, without waiting to demonstrate that this actually takes place, he inquires into the justice of the opinions of Bichat on its cause. This physiologist, it is perhaps sufficiently well known, ascribed increased afflux of blood towards any set of vessels, to the increase or augmentation of the organic sensibility of these vessels. Increased sensibility, which is precisely the same thing Dr Parry contends, denoted by pain, tenderness, itching, burning, and other unusual sensations, was considered by Bichat as the cause, not the effect, of the unusual accumulation of blood in the part. Dr Parry doubts the fact in many instances; and in those in which it must be admitted, he contends that it is not the cause, but the consequence of the increased fulness and impulse. "If a part," he remarks, "be rubbed, it is certain that it becomes fuller of blood, and its animal sensibility is increased; but it does not thence follow that the sensibility was first increased, and the fulness followed. The *sensation* indeed was; but not the *sensibility* or *capacity* of sensation."

Now we do not know that this mode of raising an objection is perfectly legitimate, or in all respects consistent with the principles of sound reasoning. The instance which Dr Parry here adduces, and in adducing which, indeed, he follows a great crowd of reasoners, is an instance of mere temporary fulness or afflux of blood. Those against the validity of which he adduces it, are examples of permanent fulness and distension of vessels; and that this is a very substantial difference,—a difference not merely in degree, but in kind,—no one surely, conversant with physiological reasonings or pathological phenomena, will be so hardy as to deny. It is this circumstance of permanency or durability in the condition of the capillary distension that forms the peculiar character of what has been termed morbid determination; and let the opposers of the theories and distinctions of Bichat say what they please, the difference is so great and so marked, that it must depend upon some quality of the capillary blood-vessels themselves. It is of no moment whether this quality be termed irritability, tonicity, or contractility; and it is not perhaps of much moment whether it be impaired or increased. These are mere names, about the propriety of which physiological authors may dispute; but we presume there can be no doubt that these capillary vessels possess some quality; and perhaps one of the most certain proofs of its existence, is the implied admission of its existence in the various names which it has received from different theorists.

Admitting the theory of morbid or irregular determination to be well established, so far as regards the mere fact of loaded vessels, and perhaps slow or languid circulation, we come to see in what manner Dr Parry applies it to explain the phenomena, and improve or illustrate the treatment of diseases. Without dwelling on the ordinary inflammatory disorders, the first point is in showing the inflammatory nature of dropsy, and its intimate connection with hemorrhage.

Rush of Philadelphia was, we believe, the first physician in modern times, who, from various proofs, symptomatic, pathological, and therapeutic, inferred the inflammatory character of many dropsical disorders. By an accurate generalization of facts, he came to the conclusion that all dropsies were of three sorts only;—the tonic or inflammatory,* in which he supposed the blood-vessels to be in a state of extreme action, or, in other words, inordinate fulness and tension;—the atonic or typhous, in which he supposed a feeble morbid action of the capillaries to take place;†—and, thirdly, a mixed sort, distinguished, according to his view, by "an *intermediate* or mixed action of the arterial system, analagous to the typhoid action which takes place in certain fevers." ‡

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* On Dropsies. Medical Inquiries and Observations, by Benjamin Rush, M.D. In four volumes. Philadelphia. 1805. Vol. ii. p. 159.

† Ibid. 176.

‡ Ibid. p. 183.

Without entering into the question of the justice of this threefold division, it is sufficient to remark, that the proofs adduced by this ingenious and original inquirer to demonstrate the connection of dropsy with inflammatory action, or inordinate fulness and tension of the vascular system, were so satisfactory, that, though received at first with doubt, and treated as extravagant theoretical innovations, they have at length been very generally adopted by almost all enlightened physicians, and are made the basis of the modern treatment of dropsical diseases. The researches of Blackall added strong confirmation to these principles; but the attempt made by this physician to connect the inflammatory character of dropsy with the presence of coagulable matter in the urine, though ingenious, is found by no means to hold universally.

It is due to the character of Dr Parry to say, that he appears to have been led, at an early period of his professional labours, to suspect the inflammatory nature of dropsical effusions. In the work before us, he draws this conclusion from the frequent occurrence of dropsy after scarlet fever, from its connection with a febrile state of the system, with acute rheumatism and hemorrhages of various kinds, either as an attendant or a sequel; and, above all, from the salutary effects which copious bleeding, either artificial or spontaneous, almost invariably exercises in removing the dropsical swellings. In the doctrine of Cullen, according to whom these swellings were often the consequences of profuse evacuations of blood, the following observations would have been regarded as highly heretical, and founded on most mistaken views of the origin and nature of disease.

"The hemorrhagic and dropsical state," says Dr Parry, "being usually the same, it is not to be wondered at, that the latter should occur to persons subject to the former. When, however, dropsy follows large hemorrhages, it does not accompany them, but supervenes when they have ceased. Hence it appears to be a collateral effect of the constitutional state, or a kind of vicarious discharge, rather than the consequences of the preceding hemorrhage. On the contrary, it is probable that hemorrhage, by removing excessive fulness of vessels, often prevents dropsy. If this be true, we may naturally expect that the converse may sometimes happen; and that hemorrhage supervening to dropsy may also cure it.

"When dropsy is associated with large artificial hemorrhages, I observe, that as in the case of hemorrhages which are spontaneous, it does not usually accompany them, but comes on after they have ceased; and I have concluded that it is the effect of nutriment absorbed too suddenly for the relative state of the vessels, which therefore strive, if I may be allowed the expression, to get rid of it by every possible outlet."

These principles are illustrated by apt cases in their proper place; and if the personal experience of any of our readers does not furnish them with examples of dropsical effusion after large and copious bloodletting, which were to be ascribed to the same cause, we recommend them to peruse those given by our author. By the same process of reasoning Dr Parry infers, that dropsical effusion is in some instances a salutary effort made by the system to relieve itself of a greater or more immediate and urgent evil, and that it is then to be viewed as a curative process. It would have perhaps been more rational and not less useful in a practical light, to regard it as one of several effects which may result from the same pathological cause, and the actual occurrence of which will depend on the energy, promptitude, and success with which that cause is removed, or prevented from producing its legitimate effects.

We have already alluded to the early period at which Dr Parry appears to have been led to entertain these opinions; and this is established in a most satisfactory manner, from the time at which he began to act upon them in the treatment of dropsical diseases, which the present work shows he did upwards of forty years ago. It would be frivolous to notice this circumstance, did it not show, that, at a period when the Cullenian doctrines were in the zenith of their reputation, this physician had ventured to deviate from the beaten track, and had with equal originality and success adopted remedial measures, which, in these days of slavish imitation, must, if generally known, have been consigned to everlasting perdition by all good and obedient disciples. We select as examples two cases,—one so early as 1784, the other in 1790, in both of which bleeding was almost the sole remedy.

" About the year 1784," says Dr Parry, " I attended a young man who had peritoneal inflammation, with a stoppage in the bowels, which were with difficulty relieved, and were immediately followed by *ascites*. The fever continuing, I caused him to be largely blooded from the arm, and ordered him saline and opening medicines. The fever soon ceased, and the *ascites* disappeared with little aid from the remedies usually exhibited as diuretics."

In this case the dropsical symptoms were cured by the use of copious artificial bleeding exclusively. In the following, the same effect followed a small artificial bleeding, and a larger spontaneous or natural one.

" Col. G. C. a short, rather corpulent man, between fifty and sixty years of age, who had been a free liver, and subject to frequent fits of the gout, became my patient in consequence of great œdematous swellings in his legs, paucity of urine, and total loss of appetite; for which, as they were supposed to be connected with a gouty diathesis, he was sent to Bath for the use of the waters. At my first visit I found him labouring under these symptoms, to which were added a sense of great heaviness in his head, a flushing of the face, disturbed sleep, an occasional stertorous inspiration while he was awake, depressed spirits, a very dry tongue, and full, strong, and rather quick pulse.

" At this time, August 1790, I was young in my profession, but not altogether unobservant of the facts which presented themselves to me; and as it had happened to me to see more than one case extremely similar to that which I have described, in which the patient either had afterwards epilepsy, or was relieved by copious hemorrhage from the nose, I ventured to predict that one or other of these events would occur to Col. C. unless they were prevented by a timely artificial evacuation of blood.

" It will readily be conceived what impression this proposition at that period of medical knowledge made on the minds of the patient and his friends. What! bleed a man at an advanced period of life, of a gouty habit, in such a state of debility, actually labouring under dropsy, and who clearly wanted blood, rather than suffered from a superfluity of it? Never surely was there so wild and extravagant a proposition.

" Notwithstanding these objections, I persevered, and was fortunate enough to gain my point. Ten ounces of blood were taken away by cupping. The patient felt immediate relief of all the more important symptoms. The weight in his head and stertorous inspiration were removed, the tongue became less dry, the pulse softer and slower, he had that night many hours of comfortable and refreshing sleep, and waked with some appetite.

" But though the remedy was beneficial, it was insufficient for the purposes of the constitution; for on the following day he was seized with a spontaneous bleeding at the nose which was very copious, and in a few days no symptom of indisposition remained."

Several other cases are related in 1799, 1804, 1805, 1808, 1809, 1810, 1811, in which the dropsical symptoms were speedily removed, either after a copious spontaneous hemorrhage, or a very full artificial blood-letting, and the patients were, without any untoward occurrence, quickly restored to steady health. And very nearly to the same effect, instances are given of the power of over-nutrition in producing dropsy, and of its removal by purging, spontaneous or artificial. (p. 207, 208).

It is long since the profession became acquainted with Dr Parry's application of this theory to the treatment of the disease termed *Purpura*, Purples, or Land-scurvy, whether with or without hemorrhage. It is well known that this disease, if it received any station whatever in the nosological system of Dr Cullen, was placed in the rank of passive hemorrhagies. If this eminent teacher did not here admit the old doctrine of putrefaction of the blood and dissolution of its fibrin, he at least gave countenance to the not less alarming one of gangrene and mortification. Nor was the sound judgment of Willan able to penetrate and dispel these mists of prejudice, though his learning and research furnished the most accurate and distinct history of the disease extant. It was to the genius of Parry that we are indebted for the removal of those errors, and the destruction of those prejudices which ascribed such pernicious effects to the use of the lancet in this disease. While according to the old doctrine of obviating gangrene and correcting putrefaction, physicians ordered liberal doses of bark, and such stimulating medicines as were supposed to possess antiseptic powers, and as ample an allowance of nutritious food and wine as the stomach and finances of the patient could bear, Dr Parry showed that the disease could be cured in a much shorter time, and with much greater certainty, by removing the febrile or inflammatory state of the sanguiferous system, by means of bloodletting, purgative medicines, and low diet. The soundness of his pathology has been confirmed by the success of the practice; and in the present day, there is scarcely a sensible practical physician who would treat the purple disease in any other mode, than by bloodletting and purging according to circumstances, and the other parts of the cooling regimen. If any one requires further confirmation of the excellence of this method of treatment, we have no choice left but to recommend to him to study the cases recorded in the work now before us.

Acute rheumatism has been admitted on all hands to be an inflammatory disease, or one, according to Dr Parry, of increased determination. Yet there is none, on the treatment of which greater difference of opinion has been entertained by the most eminent and most accomplished members of the profession. Though the blood is almost invariably very sizy and cupped,—though, whether the heart contracts more frequently or not than usual, the circulation is always much disturbed,—though all the symptoms are relieved by bloodletting, yet this remedy has been loudly and repeatedly condemned as one which alleviates without curing, and protracts to an interminable length the duration of the disease. By others again the sweating system, at one time so much extolled, is represented as a useless or even injurious practice; and the liberal administration of cold and cooling applications is commended as an unfailing remedy.

But of all the cures of ambiguous repute in this disease, none has received a greater degree of praise and obloquy than the Peruvian bark. By one party it has been extolled as a never-failing specific, fitted to every stage, and to every form almost of the disease. By others it has been censured as absurd and irrational in theory, pernicious in practice, and deleterious in effect. Though frequently employed shortly after its introduction by Morton and others in the treatment of acute rheumatism, it appears never to have acquired the general confidence of the profession, after the doubt expressed of its utility by Pringle, Cullen, and Heberden.

Pringle admits, that though he had some success in giving it early, he had not seen cases enough to recommend the practice;[*] Cullen pronounces it to be absolutely improper in acute rheumatism, and states that he found it manifestly hurtful in the beginning or inflammatory stage of the disease;[†] and Heberden classes it with remedies which must be considered as under probation.[‡] In opposition to this we have the experience of Hulse and Fothergill,[§] and the elaborate researches of Haygarth.[‖] The testimony of the two last is undoubtedly of considerable weight, as both were originally impressed with the idea so strongly inculcated by Cullen, that it must be injurious during the inflammatory state of the system, and while the blood still exhibits the buffy coat. Fothergill had seen reason, in the relaxing atmosphere of London, to accuse blood-letting, and the evacuating system in general, of protracting the disease, or inducing a fatal termination, especially in young persons—a conclusion in which he was afterwards supported by Willan and others. To prevent such results, he was induced, on the recommendation of Sir Edward Hulse, to employ bark first in his own case, and afterwards in his practice on others. From Fothergill the practice was adopted by Haygarth, to whom the profession is indebted for a full and patient practical inquiry into the anti-rheumatic powers of Peruvian bark. The reader may there find the results, and will read with interest the confident panegyric pronounced on this remedy, by the zealous and benevolent physician of Chester. The *per contra* side of this balance-sheet is shortly, but decidedly, given by Dr Parry in the following manner:

"In one instance, nearly thirty years ago, of a patient who, as sometimes happens, had many successive paroxysms of rheumatic inflammation, which lasted several days, and then left a tolerably perfect remission, the bark did certainly appear to cure the patient, after the failure of sudorifics and other remedies, such as the experience of that day had supplied.

"In another case, many years afterwards, in which the patient was cured by another process, and in which I gave the infusion of bark, with sulphuric acid, merely to remove the subsequent debility, its exhibition for a single day produced a renewal of the local inflammation. And, when scarcely crediting the connexion of circumstances, I repeated the same remedy after the cessation of the symptoms, a precisely similar effect in a few hours followed. After this second trial, I did not consider myself as justified in making a farther attempt, and the patient continued free from relapse.

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[*] Observations on the Diseases of the Army, p. 160. London, 1768.

[†] A Treatise on the Materia Medica, Vol. II. p. 100.

[‡] Gulielmi Heberden Commentarii. Editio Altera. Lond. 1808. p. 346–347.

[§] Works of John Fothergill, M. D. London, 1784. 4to. On Weather and Diseases, p. 105.; and Use of the Cortex Peruvianus, p. 240.

[‖] A Clinical History of Diseases, including Reports on Acute Rheumatism and Nodosity of the Joints, by John Haygarth, M.D. Bath and London, 1805.

" In a third instance, the patient had begun to take cinchona at the commencement of a very inflammatory attack of this malady; and at the end of ten weeks, was precisely in the same state as that in which, under other treatment, he would have been in the first week. A different plan was adopted, and the patient soon happily recovered.

" It does not indeed appear to me that the cases published plead very strongly in behalf of this remedy. The doses were so small, as surely not to have had much power over the malady, while on the other hand, in some of the cases in which it was administered, fatal symptoms appear to have supervened. This is extraordinary. We seldom hear of patients dying of acute rheumatism. In my own practice, of thirty-five years, I have never seen an example of such an event in that malady. The inference, therefore, regarding cinchona, is not by any means favourable. There is much analogy to show the tendency of that medicine to produce excessive determination to the vessels of the head. I am therefore, on the whole, disposed to consider it as having, in most cases, done nothing more than precluded the employment of the usual injurious practices; while in those of a worse kind, in which the brain was apt to partake of the general inflammatory state, this remedy, if it did not increase that disposition, at least prevented the early administration of more efficacious remedies.

" In three cases, which I have within these two last years attended, and in which the disease had previously run out to a great length, and had either become chronic, or had extremely reduced the constitution of the patient, I have found cinchona produce good effects on the general health; and I greatly doubt, whether, under any other circumstances of this malady than those which I have stated, it will in future experience be found equally beneficial with other means."

Under the head of Gout, we find queries and remarks on the difference between gout and rheumatism; observations, cases and dissections, tending to illustrate the pathological character of gout; and cases to demonstrate the propriety and the efficacy of depleting measures in conducting its treatment. It would be impossible, in the compass of this article, to enter into the consideration of a subject so extensive and so complicated as the nature of this little known disease must evidently render every inquiry concerning it to be; and we confine ourselves to the notice of an interesting case, which shows gout of the stomach to consist in genuine inflammation of that organ. The subject of it was an eminent surgeon, aged 47, who, after two fits of the gout in his lower extremities, each succeeded by rheumatism in the hands, elbows and neck, became sick, complained of heat and pain of the stomach, increased by taking either solid or fluid aliment, and at length rejected, by vomiting, more or less of what he swallowed. The matters vomited were at first brown, and clear, afterwards brown and opaque, and at length reddish, as if from admixture with blood. Notwithstanding the usual stimulants, wine, spirits, brandy, and gout cordial, the symptoms became worse, and at length terminated fatally. "The outside of the stomach had the natural appearance; but within, the villous coat was much diseased. The cardiac portion had more or less inflammation, appearing in dark, red spots, and patches of various sizes, the intervals of which diminished towards the greater curvature, where the villous coat was entirely of this colour,—not extending, however, to the muscular coat. The diseased surface, on being cut, every where poured out blood. The great end of the stomach contained about an ounce of the same reddish brown, opaque fluid, which had been rejected by vomiting."

Several cases are given which demonstrate the salutary effects of blood-letting in gout.

It was, however, more particularly in his opinions on the origin and nature of what are termed Nervous complaints, and in his practical measures in their treatment, that the speculations of Dr Parry on morbid determination were viewed in the light of fanciful conjectures, and wild and groundless innovations. It was not new or unusual to ascribe apoplexy, palsy, or epilepsy, or even water of the brain, to unusual fulness and vascular tension; nor even was there any thing extraordinary in perceiving something of this kind in insanity, and the varying shades of mental derangement, though the treatment of persons in this unhappy condition was never in the least degree conducted on this principle. But it was certainly new to ascribe all instances of dazzling of the eyes, or coruscations; giddiness; ringing of the ears; the sensation of black spots (*muscae volitantes*), floating before the sight; sick headach; tremors; that sinking and lowness which occurs in dyspeptic persons; even the nervous temperament strictly so called, or extreme mobility of the muscular system; and hysterics—to unusual determination, or rather inordinate fulness of the vessels of the brain. Many physicians of deserved eminence had been at considerable pains to explain the nature of these complaints, and to show that they arose not from immediate disorder of the brain or nervous system, but from a sympathetic weakness of this system, depending on disturbance, derangement, or interruption to the process of digestion. In proof of this, we have merely to remind our readers of the observations of Hoffmann and Whytt on Derangement of the Functions of Sight and Hearing in persons of the temperament named *nervous*,* of the interesting Essay of Dr Fothergill on Sick-headach,† of the more recent remarks on that complaint by Dr Warren,‡ of the observations of Mr Ware on *Muscæ Volitantes*,§ and various remarks by other authors, too numerous to recount. It was the great purpose of these authors to prove, that there was a real distinction between these complaints occurring in nervous persons, (we regret to be obliged to use this ambiguous term so frequently), and the same complaints appearing as precursory symptoms in apoplexy and palsy, or as attendants in continued fever and other affections of the brain; and it was further their object to show, that these complaints were most successfully removed, not by blood-letting and other evacuations, which they contended were injurious, but by tonic or strengthening medicines, —by those which improved the digestive function, and by those which were conceived to fortify and establish the nervous system.

It was in 1788 that Dr Parry first ventured to question the truth of these opinions, and that he attempted to show that nervous diseases depended on excessive determination of blood to the vessels of the brain;—a theory which, he informs us, was adopted from the evidence of facts against all the prejudices of education and of universal opinion. A doctrine so much at variance with the established principles of the day, was not likely to be either readily or unanimously admitted; it was in truth strenuously opposed, sometimes with more violence than reason; and it has since undergone the severest scrutiny, which, in the opinion of its author, has served only to confirm its truth, and establish its claims to regard.

It is inconsistent with our character to admit, without question or qualification, the assertions of any author, however respectable and however intelligent: but we are perfectly willing to concede to Dr Parry, that although some of his positions, with regard to the force with which the blood is propelled through the carotids, and concerning the alleged effects of pressure on these arteries, rest on grounds which we regard as untenable, yet this does not invalidate the general principle for which he virtually contends, that in most, perhaps all, nervous disorders, there is an inordinate fulness and tension in the vessels of the head in general, and of the brain in particular, and probably an undue fulness or plethora in the vascular system at large. In the present work he has accumulated new proofs of the truth of his theory; and he has collected a body of evidence, pathological and therapeutic, from which it would require great ingenuity to draw any other conclusions, than those which he has given. The arrangement however is, from the causes already mentioned, necessarily confused, and prevents the reader from seeing their collective force distinctly. We shall do little more in the subsequent part of this article, than attempt to show what are the principal points which Dr Parry endeavoured to establish, and what are the means which he has employed to effect his purpose.

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* Frederici Hoffmanni Medicinæ Rationalis Systemat. Tom. 4ti. &c. and, Observations on the Nature, Causes, and Cure of those Disorders which are commonly called Nervous, Hypochondriacal, or Hysteric, in the Works of Robert Whytt, M. D. 4to. Edin. 1768.

† Remarks on that complaint known under the name of Sick Headach, in Medical Observations and Inquiries, Vol. VI. p. 103. or the Works of John Fothergill, M. D. 4to. London, 1784. p. 597.

‡ Of Headachs which arise from a defective Action of the Digestive Organs. By Pelham Warren, M. D. Transactions of the College of Physicians, Vol. IV. p. 233. Lond. 1813.

§ On the Muscæ Volitantes of Nervous Persons. By James Ware, Esq. F.R.S. Medico-Chirurgical Transactions, Vol. V. p. 255.

Without attempting to enter into a minute enumeration of the various facts and arguments adduced by Dr Parry on this part of his subject, or dwelling on the many curious and useful points in pathology and practice to which they lead, it may be said, on the whole, that his principles may be reduced to the three following leading heads:—first, that nervous diseases in general depend on vascular fulness of the part or parts in which the nervous condition and symptoms take place, (pp. 266, 330, 332, 336, 337,); secondly, that increased determination or inordinate fulness is more common in the vessels of the brain than in those of any other organ, (p. 260, 267, 320, &c.); and, thirdly, that this liability of the brain to increased determination or inordinate vascular fulness gives rise to several diseases denominated nervous, which are commonly, but erroneously, supposed to be quite unconnected with the state of the circulation.

To establish the first of these propositions, Dr Parry reminds us, that in nervous complaints the symptoms of the general affection, or what he regards *as an affection* of the brain, show themselves in different parts. Thus, in the alimentary canal, they show themselves by hysteria and vomiting; in the stomach, (*Quaere*, and diaphragm), by spasmodic difficulty of breathing; in the schneiderian membrane, by catarrh or vascular fulness there; and in the forehead, eyes, and hind-head, by nervous headach, accompanied with fulness and stuffing of the nose, like the effects of cold, but coming and going off in a short time. Nervous asthma is nothing but congestion of the whole mucous membrane of the lungs, brought on by various causes. He contends that noise in the bowels of nervous patients is not caused by flatulence, because in such cases wind neither passes up nor down, but keeps pace with respiration, often following exactly every inspiration, because it is not necessarily accompanied with dyspeptic symptoms, and because it is neither aggravated nor relieved by such circumstances as increase or remove indigestion. He further adduces various arguments to prove, that there is an analogy between nervous determination to the head and inflammation of a part, (p. 333); that inflammatory and nervous affections equally arise from extraordinary stimulation; that nervous complaints have a period of exacerbation and remission similar to those of fever, (p. 331); and that many diseases called nervous are like those of an inflammatory nature, much relieved by discharges of blood. To the same purpose he makes a curious observation on the effect of corpulency occurring in persons liable to nervous ailments, which we think we have had occasion to see confirmed, though it is not invariable or universal in occurrence. Indolent persons who become fat are not so apt, he thinks, to be affected with these ailments as those who remain thin; and he considers it as highly probable, that "this preservative effect of fat is the result of the natural process of fattening, by which a state of vascular fulness is avoided, and those irregular affections of the sanguiferous system, and consequently of the nerves acted on by it, are prevented, fatness acting like any other spontaneous evacuation from the blood-vessels."

From these considerations Dr Parry infers, that all the complaints denominated nervous, appear to arise from irregular and unequal distributions of blood to different parts of the habit, according to their *excess or defect*, *increasing or diminishing* in an undue degree the functions or actions of the *parts severally affected.* And in the same spirit he represents general nervous irritability, showing itself in unjust mental depression, spasms of various parts, &c. to depend on undue sensibility of the brain, produced perhaps by a plethoric state of its vessels; while *actual* coldness of parts is owing to deficiency of blood in their vessels.

In corroboration of the second proposition, we find a great number of proofs advanced, many of which, we cannot doubt, must be familiar to our physiological friends. We have only to remind them, that these proofs are either anatomical, as the short distance and direct communication between the heart and the brain, by means of the carotids; or physiological, *e. g.* the commencement of heat after general coldness, first about the head, and afterwards of sweating; the continuance of pulsation in the carotids, when it is gone in the radial, temporal, and other remote branches of the arterial system; the pain of distention in the carotid and subclavian arteries during palpitation or violent action of the heart; delirium in the hot stage of intermittents, and of fevers in general; the flushed countenance, sparkling eye, and throbbing temples of the early stage of intoxication; and many other phenomena well known to all our readers.

It was with the view of establishing the fact of this inordinate determination more distinctly, that Dr Parry appealed to his well known experiment of compressing one or both carotid arteries; and, as the possibility and the effects of this compression have been equally called in question, he enters into an elaborate investigation of the objections advanced by his opponents, and gives a full view of the proofs and arguments on which his assertion is founded:—1st, When a rush of blood is perceived in the ears, and each gust is synchronous with the *systole* of the left ventricle of the heart, Dr Parry has been often able, by pressure on one carotid, wholly to stop the rush on that side, while no change was perceived on the other. When the pressure was removed, the gusts in a few seconds returned as before. 2d, When any degree of pain has existed about the neck and throat, strong pressure of the carotid on one side always increased the pain in the other;—an effect which Dr Parry ascribes to the greater proportion of blood hydraulically compelled to flow through the uncompressed artery, while the compressed one is almost obliterated. 3d, Sense of weight, and fulness of the head, and flushing of the face, he has often seen removed by pressure on one or both carotids. 4th, Other instances have been mentioned in which existing maladies have been for a time removed by this compression; and the effect has been in exact proportion to the precision and degree in which the artery was compressed. 5th, Dr Parry has seen hemorrhage from the nose considerably moderated by this compression. And, 6th, It has been repeatedly employed with perfect success to remove the hurry of spirits and rapid transition of thought depending on quick motion of the heart, and to induce calm sleep. After examining, at some length, the effects resulting from tying the carotid artery, by Sir Astley Cooper and Mr Travers, Dr Parry concludes with the following inference:

"On the whole, it appears to me, that the facts which I have adduced are sufficient to show, beyond all reasonable doubt, the seat and morbid condition of the parts in those diseases usually called nervous; and the mode in which that condition is relieved by the mechanical operation which has been described."

On the third proposition, that this liability of the brain to increased vascular fulness is the origin of all nervous disorders of every sort, and without exception; it is superfluous to dwell longer, after what we have already laid before our readers, on the other two. It is further almost impossible; for a large proportion of the work is occupied with numerous interesting and pointed cases, all tending to establish the great general conclusion of the ingenious author; and the only mode in which a tolerable idea of their conclusiveness can be obtained is, by consultation at least, if not studious perusal.

We trust that the specimens which we have already given will induce our readers to study the work itself, in which, we are confident, they will not be disappointed in finding much valuable information. Besides numerous cases illustrative of the opinions of Dr Parry on nervous ailments, and of his mode of treatment, they will find many good hints on hysteria, insanity, and mental derangement in general, illustrated by observations; on epilepsy, and its connection with hysteria, on the one hand, and with apoplexy on the other; on the several forms of apoplexy, and their best mode of treatment; on palsy, and its varieties,—all more or less illustrated by cases and dissections; on sundry local affections of the nervous system, as loss, derangement, or imperfection of speech, of taste, of hearing, of smell, and of vision; squinting, double vision, ocular spectra; local palsies; *sciatica*; tic douloureux; tetanus and chorea. The dissections are rather numerous, and highly interesting. Together with the treatment of the successful cases, they furnish irrefragable proof of the propriety, and the indispensable necessity of the antiphlogistic mode of treatment; and if any one still contends for the exhibition of tonics and stimulants in hysteria and epilepsy, hopes to cure tetanus and chorea by antispasmodics, or dreads the lancet in serous or nervous apoplexy and palsy, we trust that he will at least look at the cases of Dr Parry before he writes another prescription, and commune honestly and deliberately with himself, whether he could not more speedily, more certainly, and more safely cure his patients, by following the steps of that eminent physician.

To conclude, this work is a rich mine of pathological and practical information. The ore is both excellent and abundant; but it is so strangely and whimsically mixed with the dross, that it requires more leisure and patience, than skill and ingenuity, to effect the separation. Such as it is, however, it will form the source which, in skilful hands, may be made to furnish the precious materials of a new and, we may add, a more correct system of the philosophy of diseases, and from which the judicious practitioner will derive many valuable practical precepts.

VII.

Practical Remarks upon Indigestion; particularly as connected with Bilious ard Nervous Affections of the Head and other parts; including Observations upon the Disorders and Diseases of the Stomach and Superior Parts of the Alimentary Canal. Illustrated by Cases. By JOHN HOWSHIP, Assistant Surgeon to the St George's Infirmary; Member of the Royal College of Surgeons and Medico-Chirurgical Society of London; Royal Medical Society of Edinburgh; Societé Medicale D'Emulation, Paris; Membre D'Honneur de la Societé pour les Sciences Naturelles et Medicales a Dresde; Academia Caesarea Naturae Curiosorum, Bonn; and Societatis Regiae Medicae, Copenhagen, &c. &c. London, printed for Longman, Hurst, Rees, Orme, Brown, and Green, Paternoster-Row. 1825.

THE author of this little work has the merit of concentrating, within a very narrow compass, a great deal of useful instruction, both on the pathology and on the treatment of the diseases of which he treats. It consists of two divisions; the first chiefly didactic, containing general observations on the pathology and treatment of the diseases of the alimentary canal, disposed in three hundred and seventy aphoristical or narrative propositions; the second entirely practical, containing twenty-nine cases illustrating the symptoms and effects of various diseases or accidents incident to the alimentary canal, generally with, sometimes without, dissection.

The first division of the work comprehends two parts, each subdivided again, according as it considers the pathology and symptoms, or the treatment of diseases. In the first part, Mr Howship discusses the following subjects; relaxed uvula, inflammation of the tonsils, putrid sore throat, chronic tumour of the tonsils, variolous or other exanthematic inflammation of the throat, abscess and ulceration of the throat, ulceration of the larynx, and of the œsophagus; palsy, spasm, polypus, pouch of the œsophagus; spasmodic, permanent, and scirrhous stricture of the œsophagus. In the second part, which Mr Howship commences with a brief view of the organs and process of digestion in the animal world at large, the disorders and organic maladies of the stomach and upper part of the intestinal canal come under consideration, in the following order nearly.

Spasm of the stomach, sympathetic disturbance from affections of the brain, and conversely affections of the head from disorder of the stomach; dyspeptic symptoms from vitiated secretions, from external violence, from drinking and other forms of intemperance; inflammation, acute and chronic; spasmodic and gouty affection of the stomach; dyspeptic and other complaints from disorder of the liver, from retention of bile, from biliary concretions, with their occasional consequences; dyspeptic symptoms from affection of the spleen; then organic affections, strictly so called, as polypus, ulceration, scirrhus and scirrhous ulceration (cancer) of the stomach; and, lastly, capillary hemorrhage from the stomach, and that injury which results from swallowing boiling water.

Of these various diseases, of their susceptibility of removal, and of the means to be employed for that purpose, Mr Howship has given a succinct but perspicuous and instructive view. Of the greater number, examples have occurred in his own practice, and he therefore speaks with the authority and responsibility of personal experience; but he has not failed to avail himself of information from every creditable source, with the view of illustrating and enforcing the accuracy of the principles which he delivers, and the soundness of the practical precepts which he inculcates. In composing his work on this plan, he has so much condensed his matter as to render any detailed account of its contents very much out of the question. Without attempting, therefore, to give any thing like a complete view of these Practical Observations, we shall merely select one or two topics for very general remark, with the view of showing our readers what they may expect when they come to peruse it.

In the first part, one of the most important subjects that meets the attention of the surgical reader, is the chronic tumour of the tonsils, and the means of removal. This disease, which consists either in chronic enlargement or inflammation of the tonsils, or in a hardened enlargement, the result of repeated acute inflammation, may either come on slowly and gradually in the habits of relaxed subjects, chiefly women, or it may remain as the consequence of repeated attacks of acute inflammation (*Cynanche tonsillaris*, Cullen; *Paristhema*, Good). Mr Howship does not inform us what is the nature of this tumour; whether it consists chiefly of coagulable lymph interstitially effused, or of new and peculiar matter. But it is certain that it gives rise to great and serious inconvenience.

" Although the feverish symptoms be removed, the consequent tumour remains, and may not only continue as an inconvenience, but slowly increase, until without local pain, the same distressing and alarming consequences take place that have been already pointed out as the occasional attendants upon acute inflammation. Several such cases I have seen, all in females of delicate constitution, and all the result either of fever or severe cold. The most remarkable of these was one in which it was eventually necessary to prevent more serious consequences by an operation, by which the largest of the two tumours was removed; which operation, although one of considerable difficulty, succeeded most happily."

The operation is certainly not easy; and though Mr Chevalier called the attention of the profession in 1812* to a method by which he imagined it would be facilitated, we have often heard doubts expressed both of its expediency and of its practicability. For our own part, we entertain doubts of neither. Difficulties there certainly are, as there are in all operations; but these it is at once the honour and the interest of the surgeon to overcome; and we do not belong to that class of persons, who regard them as insuperable to moderate dexterity and considerable perseverance. Two modes of removing enlarged tonsils have been proposed by surgical writers, the one by ligature, the other by the knife. Mr Howship, for good and obvious reasons, prefers the former, which he has practised with perfect success. He abandons, however, the technical formality of the old method by the double *cannula*, as disagreeable and inconvenient, and trusts more to manual dexterity than mechanical or instrumental aid.

"In one instance," says he, "I removed, by operation, a very enlarged tonsil, by simply passing a ligature round its basis. The ligature used was whip-cord; a simple knot first made on one end of the cord, the end so knotted was brought forward upon the other, and a single noose upon itself, including the other, was thus made, and drawn upon it close to the first knot. The running knot was then passed along the ligature, so far as to leave a circular noose rather larger than the circumference of the tonsil to be included. The patient seated in a proper light, the head held steady, and the tongue depressed, I carried the noose of the ligature in my left hand through the mouth nearly to its place, and settled it accurately round the basis of the tumour by the assistance of a proper instrument. Then an iron instrument with a small ring at its end, adapted to push along the running knot, was passed along the ligature to the point where it reached the basis of the tumour. The instrument now held steadily, its handle being the fulcrum, the ligature was very cautiously and firmly drawn outwards, so as by slow degrees to push the knot closer and closer without disturbing the position of the noose, till no more could be gained; the tightening of the ligature occupied near five minutes. The instrument removed, the end of the ligature was attached to the angle of the mouth by sticking-plaster, and the operation was finished."

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* Medico-Chirurgical Transactions, Vol. III. p. 80.

Some additional particulars will be found in the account of the second case, p. 146, in which this operation was performed by Mr Howship with the assistance of Mr Heaviside and Dr Harrison. This operation differs from the one described by Mr Chevalier in the third volume of the Medico-Churargical Transactions, only in not perforating the basis of the tumour, and in the ligature being drawn to the requisite tightness at once, instead of gradually. It is sufficiently simple, and we are assured by Mr Howship that it was perfectly successful.

The account of ulceration of the larynx is partial only and rather imperfect. In compensation, however, Mr Howship gives a good case with dissection, in which the membrane covering the arytenoid cartilages, and the upper part of the cavity of the larynx, was greatly thickened, and highly vascular, secreting matter and exhibiting the appearance of ulceration, though modified by the peculiar texture of the surface. The thyroid cartilage also was so firmly ossified that "two knives were broken in the attempt to cut it through; and in order to inspect the cavity of the larynx, it was found necessary to divide it with a saw."

The subject of diseases of the œsophagus is treated more fully; and the observations on the pathology, illustrated as it is by several interesting cases, will be perused with advantage. The most defective part of this department of the work of Mr Howship, is in our opinion the medical treatment; and this observation applies not only to Mr Howship individually, but to the practice of surgeons in general in these diseases. It seems to be a general belief, that diseases of the œsophagus, such as ulceration, spasm, stricture, &c., admit of relief by local means only; and that all remedies which operate through the medium of the constitution or of the functions, are inert and frivolous. They seem to be unaware, that, though inflammation or local congestion is not always the cause either of spasm or of stricture, yet it will be found to make so important a part of both, that the means of abating or subduing them, will be found in all cases to be the means of alleviating, or it may be of favouring the removal of a disease supposed to be organic. We must further remark, that every case of ulceration is both preceded and accompanied with marks, however obscure, of local inflammation; and that its degree and severity is always proportional to the violence of this local inflammation. It is for these reasons, that, in every case either of ulceration, or of stricture of the œsophagus, whether proceeding from spasm, or, as it often does, from chronic inflammation of the mucous membrane or its follicles, or even from scirrhous disorganization, the utmost attention ought to be paid to the management of the digestive function, and its dependent processes of secretion and excretion; and local determination of blood ought to be obviated as much as possible by leeches, or by cupping between the shoulders, repeated according to its effects on the symptoms. There are even cases in which a general bleeding from the system may be requisite; but there are few in which occasional or periodical local bleedings can be dispensed with. In a future edition of his work, we suggest to Mr Howship the propriety of inserting a few specific directions on this subject in his 117th paragraph. He has not omitted to notice in the subsequent one the necessity of attending to the state of the bowels.

In the second part, relating more immediately to disorders of the functions and change in the structure of the stomach, Mr Howship ranks, under the head of sympathetic disturbance of the stomach from affections of the brain, the confusion of head, general weakness, and consequent vomiting of sea-sickness, and vomiting from injury of the head. The former of these opinions, he tells us, he has been led to adopt from personal observation of his own feelings, and of the complaints of others, when much at sea at one period of his life. He does not appear, however, to have been more successful than others in the discovery of an unfailing remedy for this distressing condition. Wine, spirits and water, and all the usual palliatives, as ether, effervescing draughts, &c. he mentions; but none of these are invariably successful. He however adduces a good example of the effect of strong emotion in removing this disorder in his own person, which deserves notice.

"On one occasion at sea, in heavy weather, I was myself extremely ill, sick, and lying down; a sudden noise and commotion upon deck drew my attention, when one of the officers running into the cabin, told me the hold of the ship was all in flames. The more powerful impression in an instant took the lead; I jumped up, and although, almost by a miracle, the fire was extinguished, and the ship and all the lives providentially saved, I from that moment felt neither headach nor sickness."

Conversely, disorders of the functions of the stomach may give rise to disorders of the head; and Mr Howship is one of those who, unlike Dr Parry, ascribe swimming of the head, giddiness, and the nervous complaint termed *sick-headach*, entirely to imperfect or disturbed digestion. We have had occasion, in another article, to bring this subject under the review of our readers; and between Fothergill, Warren, Ware, and

Mr Howship, on the one hand, and the strong facts adduced by Dr Parry on the other, we feel perfectly incompetent to decide on the ultimate merits of the question. In support of the general opinion, Mr Howship speaks certainly with much reason, and with the additional argument of four cases, the three first of which illustrates the power of bad digestion in inducing giddiness, swimming of the head, and general languor; and the second shows the possibility of the same cause giving rise to threatening apoplexy.

"Many such cases," says Mr Howship, "I have had under my care, where a distressing swimming in the head and alarming giddiness, or habitual attacks of most severe headach, were only attended with an apathy of stomach, or total absence of desire for food, and want of power to digest it if taken. In one of these cases a gentleman had for this complaint long been in the habit of frequently losing blood by cupping, under the direction of his physician, and felt at last as uneasy at the idea of the remedy as of the disease. By a course of medical and surgical treatment, to restore power to the stomach and action to the bowels, the complaint in the head was perfectly removed; and from that time to this (six years) he has had, I believe, no return. In another, which, in severity at least, was similar to the above, the correction of the habitual derangement of the stomach and bowels, and the removal of a disease from the lower extremity of that canal, entirely cured a complaint in the head of many years standing, which had frequently confined the patient to his bed for days together."

Those who maintain the gastric origin of complaints of this description contend, that blood-letting, and the whole of the depleting system, is not only useless but injurious; and they maintain, that however alarming these symptoms may be, by their resemblance to apoplectic seizure, they may be in all cases removed, by improving the circulation and secretions of the stomach and bowels.

"A lady," says Mr Howship, "almost unable to walk across a room without falling from this cause, observed to me, she had been advised to lose blood, but that experience had satisfied her that would not answer, having on a former occasion applied leeches, when her giddiness became suddenly so much more alarming, that words could convey no idea of what she then suffered for several successive days."

Mr Howship, however, is not a bigoted advocate of the mere nervous sympathetic effects of the gastric functions, on the cerebral circulation. While he inculcates the facility of controlling these complaints in their origin, by mere attention to the regular state of the alimentary canal, he admits that, if they do not receive a seasonable check, they may lay the foundation of serious disease of the brain.

"The concomitant derangement," he remarks, "in the functions of the brain, however, is of a more serious character;—a character that I have traced from so many different points of view, as to ascertain pretty clearly, that if, from long inattention to the state of the stomach, the disorder in the head continues to make progress beyond a certain point, it becomes a case of oppressed brain; the circulation within the head languishes, the memory fails, irresistible drowsiness supervenes, in the midst of which the senses are still watchful, till at length the patient is carried off by the establishment of serous apoplexy."

The practical inference from such facts as this, which we believe are not entirely unknown to many of our readers, is, that it matters little, whence the apoplectic symptoms proceed—whether from general fulness of the vascular system, or from disorder of the alimentary function, if they are present at all. The medical attendant, in such circumstances, must not delay in making nice distinctions about the nature of the remote cause, but proceed to employ, with activity and promptitude, the usual means of relieving the apoplectic symptoms.

The influence of disordered digestion is not confined to the circulation and function of the brain alone. It may operate on the heart, producing palpitation more or less violent; on the lungs producing asthma in various degrees; and on the renal circulation, producing more or less derangement in the secretion of urine. Each of these disorders is shortly discussed by Mr Howship.

A curious and highly important subject is merely mentioned by Mr Howship in the text of his observations, (174, p. 69.) We allude to the effects which take place in the organs of sensation and motion, in consequence of the habitual use of spirituous liquors, and which have been so well described under the name of *Delirium tremens*, by Dr Sutton and Dr Armstrong. We regard this subject as important, in consequence of the readiness with which it may be confounded with phrensy or cerebral inflammation, in consequence of the alleged difference between it and that affection, and in consequence of the modes of treatment recommended by Dr Armstrong to remove the brain-fever of intoxication, being extremely opposite to the established practice of treating phrenitic inflammation. We regard the nature of this disorder as still unknown, and the treatment as established on no certain principle. We have known more than one case very distinctly marked treated as *phrenitis*, with copious blood-letting and evacuation, with increase, rather than abatement of the spectral hallucinations, and finally terminating in death; but as we know not whether the fatal event was to be ascribed to too little or to too much of this practice, it is undoubtedly the safest course to delay giving any decided opinion. The practice recommended by the experience of Sutton and Armstrong is entirely opposite; but whether it is the best, whether it is suited to very mild cases only, a certain number and kind of these cases being always, and *ab origine* fatal, or whether the essence of the practice consists in simply withholding the stimulus of the violent and unnatural agent which induces the disorder, we feel at present quite unable to say.

Is this disorder capable of being elucidated by dissection? Armstrong regretted the want of this source of knowledge. Mr Howship informs us that he has had repeated opportunities of ascertaining with precision the condition of the brain, and other viscera after death from this cause; but he has given the particulars of three cases only, the 15th, 16th, and 17th, which he trusts may throw some light on this important disease. In each of these cases, the veins of the pia mater were full of pale, watery blood, and numerous globules of air; there was more or less serous effusion between this membrane and the arachnoid; and more or less serous fluid (from one to three ounces) in the lateral ventricles. These facts throw no great light on the nature of this disease. They are rather the effects of morbid action than the causes.

If we could venture to speculate on a subject on which several writers have speculated unsuccessfully, we would say, that the part of the disorder, in which the brain, and the organs of sensation and motion were concerned, consisted in impeded circulation, and the imperfect supply of arterial blood, rather than in any thing like inflammation. The first effects of intoxication are to destroy the equal and uniform distribution of the blood,—a fact of which any one may satisfy himself, by observing the pale contracted features, the diminished temperature, and the incapacity to preserve the natural heat of the system,—of a person in the confirmed stage of intoxication,—previous to complete insensibility. To this the increased frequency of the contractions of the heart is no objection; for they are, unless when aided by external heat, smaller and weaker than in the natural state; and, in short, every circumstance of this condition of the system may be justly said to denote impaired power of the heart to propel its blood; a languid vacillating state of the circulation in general; and a very imperfect distribution of blood to the brain in particular.

We would further remark in confirmation of this fact, that we have known the same spectral hallucinations, and the same extreme mobility and tremulousness of the nervous and muscular system, take place more than once in consequence of extreme losses of blood, artificially induced for the removal of diseases unconnected with disease of the brain *e. g.* in *pneumonia*, and in the sudden and enormous discharge of uterine hemorrhage. There appears, along with this state of the nervous agents of the circulating function, to be a morbid fulness and distension of the smaller veins; but whether this is or is not the effect of the disordered state of the heart, we feel unable to say. It is not improbable, that it is to this venous congestion, as it may be termed, that we are to ascribe the gradual effusion of serous fluid which is found, as Mr Howship shows, in the ventricles, and beneath the arachnoid membrane after death.

Mr Howship proposes nothing new or particular for the removal of the brain fever of ebriety.

Gouty action affecting the stomach, Mr Howship ascribes to spasm principally, and therefore regards it as incapable of being made the subject of anatomical proof after death; and in support of his opinion, adduces the authority of Dr Baillie and of Dr Hooper.

"Upon this point," he informs us, "Dr HOOPER has in conversation acquainted me, that he does not conceive any spasmodic affections of stomach attributable to gout can ever be traced after death as such; for that the stomach in gouty habits is usually flatulent and very relaxed; and that although a person from retrocedent gout may be seized with violent and terrible pain, referred to the stomach, with vomiting of a coffee-ground-like matter, even proving suddenly fatal, almost as if killed by a canon-shot, yet the stomach has been found uniformly relaxed, exhibiting only patches of increased capillary vascularity on its internal surface. This he has himself seen in several such cases; and upon mentioning the matter to the late Dr BAILLIE, he observed, he believed it was so, and that he had seen much the same appearances, and those only. Dr HOOPER additionally remarked, that the violence of the pain was not positively known to be seated in the stomach, although there was much reason to believe it to be so, for the semilunar ganglion might be its seat; and that where muscular contraction without structural disease is found, it may be considered the natural, or at least the accidental state of the part, unconnected with any effect of disease."

This is so much at variance with the results of the observations of Dr Parry, to which we have already had occasion to call the attention of our readers, that we must still regard it as uncertain, whether gout induces spasms only in the stomach, and would entertain some doubt regarding the propriety of giving stimulants invariably and in every case in which that noble organ was attacked with gouty action. Our readers who wish further information, will do well to study the strong facts of the case, and dissections and observations of Dr Parry, in comparison with what Dr Howship has said, and especially with the perusal of his twentieth case, which is entitled, *Gouty Spasm at the Stomach*; and we think, they will find little difficulty in concluding, that gout may produce not only spasm, but more frequently inflammation when it falls on the stomach; that in most cases of gouty pain of the epigastric region, however similar to spasm it may appear, it will be requisite to be somewhat cautious in the administration of such stimulants as brandy, ether, &c.; and that a full bleeding from the system previous to the use of the opiates and antispasmodics so generally and liberally administered, will do no sort of harm.

In treating of the dyspeptic symptoms arising from diseased liver and gall-bladder, Mr Howship follows Dr Heberden too closely in his history of the symptoms of obstructed gall ducts from biliary concretions; but in compensation, he has referred to the cases of Mr Thomas and Mr Brayne, published in the Medico-Chirurgical Transactions. These examples show that the symptoms of this painful disorder are not in all cases the same as those described by authors; and that they are liable to considerable variation from the size and shape of the concretion, and its relation to the capacity of the ducts; from the susceptibility of the subject to inflammation or not; and not a little from the treatment employed. They may assume the appearance of ileus, or of intestinal inflammation in some severe and protracted cases.

Mr Howship has treated at considerable length of dyspeptic symptoms arising from disease of the spleen; and he adduces several cases in which the practical reader will find good instruction. The observations of Mr Howship on this subject should be compared with those of Dr Bree on Tumid Spleen, in the second and third volumes of the Medico-Chirurgical Transactions.

In noticing briefly the effects of the accidental swallowing of boiling water, Mr Howship differs from Dr Marshall Hall as to the actual nature of the injury and the cause of death. The latter, it is well known, thinks that boiling water is not conveyed into the stomach in this case, but is arrested by spasms of the glottis and larynx, where it excites inflammation, and eventually causes suffocation.

"The dissection of one of the cases," says Mr Howship, "presented appearances supporting this opinion; but each of the instances now brought forward will, I think, warrant an opposite conclusion. In the favourable case, it appears to me the extreme tenderness at the epigastrium can only be explained by admitting that the fluid had reached the stomach; and in the dissection of the fatal case, the inner membrane of that organ was, for some distance round the œsophagus, lined with effused lymph.

"In another case also related, together with several instances of injury to the stomach, in a former work, where a young woman purposely drank boiling water, I had an opportunity of examining the stomach while recent, and found its inner surface gangrenous, and nearly black from violent inflammation. It appears to me of the utmost importance to be correct upon this point, as our practice must be entirely regulated by what we believe to be the seat of the injury; and although the larynx may, and perhaps in every case does suffer, as well as the œsophagus and stomach, I cannot but feel convinced the latter parts claim our first regard, as being, in probability, the principal seat of injury."

From these specimens of this little Treatise, our readers will be enabled to form a tolerably correct opinion of its general merits. After what we have already said, they may be summed up in this character, that, like all the other writings of this intelligent surgeon, the subjects are of great practical importance, and they are treated in such a manner as to bear directly on the best and most improved method of removing them.

VIII.

A Practical Treatise on the Arterial System. Intended to illustrate the Importance of Studying the Anastomoses, in reference to the Rationale of the New Operation for Aneurism; and the Surgical Treatment of Hemorrhage. With Original Coloured Plans. By THOMAS TURNER, Member of the Royal College of Surgeons of London; Lecturer on Anatomy; and Author of "Outlines of a System of Medico-Chirurgical Education," &c. &c. &c. London. Printed for Longman, Hurst, Rees, Orme, Brown and Green; and J. Aston, Manchester. 1825.

THE profound and elaborate researches of Scarpa on the numerous mutual communications of the arterial system, have shed so much light on the means which Nature occasionally employs for the removal of aneurism, that a subject which, at the beginning of the present century, was at once the most obscure in its nature, and perhaps the most formidable in its effects of any in surgery, has now become the simplest and the clearest, and that upon the management of which the surgeon can calculate with the greatest precision. The practical effect of the learned Essays of the Italian anatomist was afterwards powerfully aided by the excellent treatise of Mr Hodgson, and, above all, by the bold and judicious example of several English surgeons, who carried the principles so well explained into effect. The operations performed by Abernethy, Sir Astley Cooper, Cline, Lawrence, and Colles, have proved to be so many experiments demonstrating the truth and the practicability of the views of Scarpa.

The views which have thus led to results so beneficial, cannot be too well known by every one who professes to understand the principles, and to practise the rules of surgery; and it is not only expedient, but indispensably necessary in studying the principles of the cure of aneurism, to be aware of the extensive resources provided for this purpose by the collateral circulation. It is chiefly with the view of explaining this beautiful arrangement of nature that the present little work has been composed; and it is because it explains it, upon the whole, distinctly and agreeably, that we think it deserving the attention of our readers. A book on aneurism merely, after the learned and interesting inquiry of Hodgson, would require to be admirably done indeed, in order to have any chance of claiming attention. With this author, therefore, Mr Turner has not attempted to break a lance; but with more moderate, though not less useful views, he has confined himself to the exposition of that anatomical distribution of vessels, on which the cure of aneurism, spontaneous or artificial, depends. His book may be regarded as the consideration of the limits within which nature has circumscribed the resources of the arterial system.

The work is divided into three sections; in the first of which the author enumerates, shortly and aphoristically the principal properties, anatomical, physiological, and pathological, of arteries; in the second he explains the origin, course, and distribution of the arteries of the human body; and, in the third, he treats of the practical application of the study of the anastomosing communications to the surgical treatment of aneurism and hemorrhage.

The first two sections are necessarily brief, as they are merely introductions to the third, in which Mr Turner enters on the consideration of the more immediate subject of his work, which he treats in a manner sufficiently minute and comprehensive. The manner in which he proceeds to investigate the subject will be easily understood from the following short extract:

"An inquiring student will not rest satisfied with being told, that an artery may be surrounded and closed with a ligature, and that the circulation can be carried on afterwards; but he will ask, how this is accomplished? and what the resources of nature are, when her regular functions are thus interfered with?—To answer these questions, he must know, that arteries, besides ending in veins, cells, and otherwise, have a mode of termination by anastomoses, or inosculations, *id est*, by communicating with each other:—it is by means of this arrangement of vessels, that the economy of the limb is preserved, in case of local pressure; and that the circulation is continued, when the main artery is rendered impervious by accident or disease.

"If a ligature be applied to a main trunk (as the femoral), it is obvious that the lower extremity would perish, if the blood could not pass by collateral vessels,—that is, by means of branches originating from the trunk above, uniting with some of those which arise from the trunk, below the ligature. Thus—it is by the anastomosing, inosculating, or communicating branches, that fatal consequences are prevented; and though these branches are small at first, they will in time increase in size, so as to supply the place of the original trunk. This fact has been long known; but we are indebted to modern surgery for introducing the scientific application of it to operative surgery—and for the refutation of the erroneous opinion entertained by the ancients, that when a main artery is obstructed, the limb must perish for want of nourishment." pp. 29-32.

The whole inquiry, in short, resolves itself into the application of a very simple and obvious hydraulic principle, well known both to theoretical reasoners and practical observers, of the motion of fluids in confined tubes. If, among any system of communicating tubes conveying fluids, one or more become obstructed, the fluid which was, previous to the obstruction, conveyed through them, passes after the obstruction through the others. To this principle in hydraulics is to be added the peculiar accommodating power with which the vessels of the living body are endowed, and by virtue of which they may be distended and enlarged to a remarkable degree, and sufficient to answer the great purposes of the circulation. It is in this manner that the small arteries, termed *collateral branches*, become gradually enlarged, when the neighbouring arterial trunk is obstructed, and finally attain a size and capacity sufficient to maintain the regular distribution of blood through the parts, which the immediate branches of the main trunk originally supplied.

This arrangement and its effects are described by Mr Turner, when treating of the collateral and anastomosing branches of the carotid artery, in the following terms:

"The anatomy of the blood-vessels shows, that the arteries which furnish blood to one side of the neck, face, scalp, and brain, freely anastomose with those of the other; hence, one of the carotid arteries may be obliterated with almost as little danger as the main trunk of either of the extremities. When a ligature is placed on one of the common carotids, an obstruction is set up in one of the four large arteries which transmit the blood to the brain. By this operation we should cut off the supply from one-half of the neck, one-half of the face and scalp, and one-half of the anterior and middle lobes of the brain,—if the branches arising from the external and internal carotids, on one side, did not communicate with those of the other, the internal carotids with the vertebral, and some of those of the external carotid artery.

" As it is, we see a direct route for the blood into the respective branches of the external and internal carotids, although the main channel (the common carotid) should be obstructed. First, the common carotid of the opposite side (E) is open; it divides into the internal (F), and external carotid (G). The first branch of the external carotid is the superior thyroideal (5), which anastomoses with the opposite superior thyroideal; and this is one channel by which the blood can arrive at the external carotid on the obstructed side.—The other arteries of the external carotid of one side inosculate with those of the opposite trunk; the facial with the facial, the temporal with the temporal, &c.

" Besides the communications between the branches of the external carotids, the subclavian arteries send off important auxiliary branches, namely—the vertebral (11), which in their course distribute twigs to the deep muscles of the neck, and anastomose with the cervical and occipital branches;—the inferior thyroideal (12), which inosculates with the opposite inferior thyroideal and superior thyroideal of the same side:—and the *cervicales* (14), which unite with branches of the occipital and deep muscular twigs of the vertebral.

" Thus the channels open for the passage of the blood to the neck, face, and scalp, are, the united common carotid—the vertebral arteries—the inferior thyroideal—and the *rami cervicales.*

" Within the cranium, the inosculations between the branches of the internal carotids are numerous and free, and the carotids anastomose with the branches of the basilar, which is formed by the coalition of the two vertebral arteries:—thus a particular connexion is established between the arteries of the anterior and posterior portions of the brain. The anastomoses mentioned is so free, that if coarse injection be thrown into one carotid, it will not only fill all the arteries of the brain, but will pass down the opposite carotid and the vertebral into the aorta and subclavian arteries; and if wax be thrown in by the subclavian, it will circulate through the brain, and return by the opposite vertebral and the common carotids." pp. 71-74.

Mr Turner examines successively the circumstances which render the operation of applying a ligature for aneurism practicable or impracticable, expedient or inexpedient, in the case of the aorta, the *arteria innominata*, the common carotid, the subclavian and axillary, the brachial, the radial and ulnar, the common iliac artery, the internal iliac, the external iliac, the femoral and popliteal, and, lastly, the tibial arteries. The investigation is conducted with deliberation and judgment; and the conclusions which he draws are illustrated and enforced by frequent appeals to cases in which the respective operations have been performed. Mr Turner has also added to the interest and value of his treatise, by the introduction of several cases not previously published, communicated to him by the kindness of professional friends.

On the whole, we regard this short view as a favourable specimen of the research and judgment of its author; and we are of opinion, that the surgical reader will find it by no means unworthy of his attention.

IX.

1. *An Act for better regulating the practice of Apothecaries throughout England and Wales. July* 1815.
2. *An Act to amend and explain an Act of the Fifty-fifth year of his late Majesty, for better regulating the practice of Apothecaries throughout England and Wales. July* 1825.

THOSE who are interested in medical legislation must be aware, that an Act passed the British Parliament last summer, the virtual effect of which is to vest in a company of Apothecaries the sole possession of the general practice of Medicine and Surgery throughout England and Wales, except within a few miles of London.

In order that our extra-professional readers may see at the outset the magnitude of this measure, they must understand, that by general practice is meant that union of the practice of medicine, surgery, and sometimes pharmacy and midwifery, which forms the profession of the Junior Fellows of the College of Physicians, and almost the whole Fellows of the College of Surgeons in Edinburgh, of a great proportion of the graduates of Edinburgh and Glasgow,—of the licensed Surgeons of the Colleges of London, Edinburgh, Glasgow, and Dublin;—and last and least, of the thoroughbred English Apothecary.

One would imagine that good and substantial cause must have been seen, for making over to the least qualified of these practitioners rights, which long usage had shared between them and their betters. Yet neither in the opinion of any other branch of the profession, except the apothecaries themselves, nor from their own showing in the preamble of their bill brought into the House of Commons in 1813, does it appear that a strong reason exists for a corporate association at all, much less for one which cannot be called by any name so properly as by that of a monopolizing company. And after bestowing no little pains on the subject, we have been unable to discover a better reason for the passing of the bill, than its efficient cause; to wit, the existence of a House of Commons, in which it is possible to wheedle over a few resolute votes, and so carry through Parliament a measure whose nature, tendency, and existence, are unknown to the great majority of the members.

Some people indeed have hinted to us, that the present may be one of a system of measures lately enacted, which are supposed to show on the part of the English members a disregard of the prepossessions, feelings, interests, and claims of Scotland. But we are most unwilling to admit any such connexion or even any such disregard at all. It is no doubt true that the Apothecaries' Act inflicts a far more vital wound on this than on any other part of the empire. It is also highly probable, to say the least of it, that the Apothecaries knew that very well. For, while they exhausted themselves in professions of openness and liberality to the chartered English corporations, who they felt could, in the case of interference or presumption, annihilate them in a twinkling,—they breathed not a whisper of their proceedings to the Scottish Universities and Colleges, whose interest is much more deeply at stake, and much more difficult to guard. But we are ready to believe that Parliament knew little of all these things; that it laboured under the misconception that the measure concerned the interests of England only; and that it will listen to our complaints whenever we have an opportunity of submitting them. We believe so; because when the apothecaries were caught in the act of smuggling their Amended Bill last summer, the House resolved it should continue in force for one year only, and that a fair inquiry should be made during the present session. We take up the subject, therefore, in the hope that we may be able to make some statements, which cannot with decency be propounded so broadly by our Colleges and Universities, and which at the same time may convey the news to their old pupils now scattered over England, and rouse them to stand forth each in defence of his old seat of learning.

As the following exposure may not be very palatable to the

Association, a distinct boundary must be drawn, before we proceed, between itself and its members. We have the honour to be acquainted with some of them, and have heard of others, who are ornaments to their profession, and for whom we entertain every respect. But few of these are of the true breed of English apothecary, such as the Bill of the Association is intended to perpetuate. If any of them should feel hurt at what may be said of the Association, we shall be sorry for it,—sorry that we must offend them, and doubly sorry that we have been mistaken in that part of our opinion which gave them credit for liberal views and enlightened understandings.

That the object of the Associated Apothecaries may be distinctly seen, it is right to trace shortly the history of their whole proceedings from first to last; which we are enabled to do in consequence of their Committee (of 40 chosen men) having, with singular simplicity, but not till they carried their point, published a minute account of their whole privy transactions. It is varnished all over, indeed, with a fair compound of public-spirited motives, without a single grain of self-interestedness. But if this covering is stripped off, the facts will speak very plainly of themselves, even without such a colouring as we might be justly entitled to substitute.

In 1813 they applied for a Bill to form themselves into a chartered company, with power to practise medicine, surgery and midwifery; to examine and grant licenses for such practice; to prosecute and root out all who practised without their license; to examine and license assistants to practitioners, applicants for apprenticeships, chemists and druggists, apothecary's journeymen, and female midwives; to settle among themselves by bye-laws what course of education should be required of candidates; and the only bodies whose rights were to be reserved were the London Colleges of Physicians and Surgeons, and Society of Apothecaries (whose power extends only a few miles round London), and full surgeons or apothecaries in the Royal army and navy; while Scottish and Irish practitioners of every denomination, members of the London College of Surgeons, not resident within its bounds, assistant-surgeons in the King's Army and Navy, all medical officers of the East India Company's service, in common with quacks and female midwives, were to be driven in one herd off the rich pasturage of England. Some trifling amendments were subsequently made on this bill when it was brought into Parliament; but even in its amended state it was universally disliked; and it was therefore withdrawn after the first reading.

From that time for two years afterwards, the Association had a good deal of coquetting with the Colleges of Physicians and Surgeons, and the Apothecary's Society of London; and this naturally ended in a union with its namesake, by sufferance rather than by approval of the two Colleges. In consequence, a bill was eventually passed in 1815, extending the dominion of the London Society of Apothecaries over England and Wales, and empowering them—to search apothecaries' shops for bad drugs,—to examine and license every person who intended practising either as principal or assistant after the fashion of an English apothecary,—and to prosecute and fine all interlopers. The qualifications of candidates were besides laid down in the Bill as consisting of a five years' apprenticeship to an apothecary, with such farther studies as the Society might please to account "a sufficient medical education." And the only rights reserved were in favour of the two London Colleges, the two English Universities, and the Chemists and Druggists.

"Thus," observe their Council, "thus terminated our arduous but unsatisfactory struggle,—thus did the bill pass, but shorn indeed of its fair proportions!" With such a specimen of their ideas of symmetry before it, with a bill of "fair proportions," which frightened at the very first sight even the House of Commons, the Legislature might have known what the taste of the Association was, and have kept a closer watch over the harsh creations of their study. They have been compelled, indeed, and that most reluctantly, to soften down the asperities which offended the eye of the chemist and druggist, as well as that of the English Colleges and Universities. But in the view of every one else their bill is as deformed as before, and appears as unlikely to benefit English medical science and practice in any way whatsoever. In the amended act of last year, which it is intended if possible to make perpetual in the present Session, a reservation has been introduced in favour of Surgeons and assistant-Surgeons of the Royal Army or Navy, or of the East India Company's service, and likewise of army apothecaries; who are all to be placed on a footing with the licensed members of the Society. And the Society is farther enjoined to admit, as equivalent to its own apprenticeships, five years' service with a member of any of the three Colleges of Surgeons. The first clause is certainly a great boon, and could not have been wrung from them without many a groan; but the other is a dead letter, so far at least as concerns Scotland, as *five* years' apprenticeships are very little known here; and therefore their bill is almost as ill-favoured and hurtful as ever.

Let us mark, in the first place, the reasons of State which call for a " Society of Apothecaries of England and Wales." The preamble of their original bill sets forth, besides other more vague statements,—that the character of the English apothecary is degraded by quacks and midwives usurping it,—that *by reason of this degradation* " few apprentices are obtained, and the general state of the apothecary's profession is necessarily becoming worse, and the medical departments attached to his Majesty's army and navy and colonies and the merchant service, which have been principally supplied with practitioners brought up under apothecaries and surgeon-apothecaries, have suffered, and are likely still more to suffer, from the increasing difficulty of procuring practitioners properly educated for such departments."

This cunning climax seems to have had the effect they looked for. There has hardly been a period in Parliamentary history when the Legislature welcomed so warmly any project for bettering the army, navy, colonies, and merchant service, as at the time the bill was introduced. To have all this done at one sweep was too tempting an offer to be resisted; and the apothecaries therefore did well to put it in the front of their petition. But were the facts proved before the two Houses? Did the apothecaries show that they chiefly supplied the State with medical officers; that the medical establishments of our armies and navies were losing character, and that they were losing character by sympathy, because the apothecaries were losing theirs? It is impossible: There never was a more barefaced imposition practised on the credulity of Parliament. It is a notorious fact, that our public medical establishments were at that time the pride and boast of our country, and that they never have been higher in repute than between 1813 and 1821, in which last year for the first time the Apothecaries' Act could possibly affect them. It is notorious, that they were filled up in a great measure, not by apothecaries, but by gentlemen educated at the academical and surgical schools of London, Edinburgh, Glasgow, and Dublin,—that a great proportion of the medical officers who have distinguished themselves were bred in Scotland,—that whatever was gained (and nothing ever was lost) to their character flowed from the increased qualifications required by these bodies and by the Medical Boards,—that no part of the improvement could be traced to the apothecaries, as indeed was impossible when by their own confession they were becoming every day more and more degraded. It is notorious, that the number of practitioners sent yearly from the above-mentioned schools could have supplied the state with at least twice the necessary number of gentlemen both able and willing for the service; and that, although every apothecary's apprentice who had risen to employment in the medical service of the country had been cashiered in one day, their places would have been filled with better men in a month.

The association complains that the character of the English apothecary is degraded by unqualified people usurping it. And pray is a legislative enactment the right remedy for that evil? Will not the true apothecary be better advertized to the public by inward worth and substantial endowments than by outward paraphernalia and legislative bills? In short,—for the question amounts to this,—can quacks be put down by act of parliament? Never.—Never at least by the establishment of a monopolizing company. There is an abundance of such persons in the regular profession; they abound even among well-bred apothecaries; and the Apothecary's Act, coupled with their narrow, illiberal bye-laws, is calculated much less to replace quacks with apothecaries, than to transform apothecaries into quacks.

But we cannot get apprentices, they add. It is no doubt very useful to the apothecaries to have apprentices. They cost less than journeymen; or rather are a source of profit; and they do the shop-work almost as well. The young ones are good for running messages and compounding pills; the elder ones for relieving their masters of poor patients, dressing sore legs, holding the tray at operations, and chastising their juniors. But the apothecaries likewise take it for granted that these are all very profitable occupations for a young man during the five first years of his medical studies, and that the only reason for their being less coveted than formerly is an unwillingness on the part of the student to put himself under a master who has been insulted by quacks and midwives usurping his character and profession. Did it never strike their voters in the Commons, with Mr Brougham at their head, that apothecaries' apprenticeships were falling in demand,—because parents had unluckily found out, that their children could get higher qualifications and a more respectable rank in society by spending their youth very differently? Did no member of the association ever happen to dream, that the great cause of his lessening respectability is his own narrow, trade-like education?

We are thus led in the next instance to inquire what provision the Act and the Society's bye-laws together are to make for the breeding of apothecaries,—in other words, what is the Society's notion of a well-educated general practitioner? The Act requires, that every candidate for examination shall have served a five years' apprenticeship, and received "a sufficient medical education;" and the Society defines a sufficient medical education to consist in a knowledge of Latin; in attendance on Lectures on Anatomy and Physiology, (two courses), the Theory and Practice of Medicine, (two courses), Chemistry, (one course), Materia Medica (one course); and lastly, in frequenting the medical practice of an hospital for six months, or of a dispensary for nine. It is necessary to add, that in London a *course* means a course of two or three lectures a week for four months; and that two are delivered successively between October and May inclusive.

As the candidate may and therefore generally will attend these four courses in a single season, it is evident that the Society propose teaching their youth chiefly by apprenticeship. Accordingly, in a paper written for them in their Transactions by a Mr Alcock, one of their number, a very pleasing picture is drawn of a well-spent apprenticeship. Mr Alcock's abstract of an apprentice enters his ideal shop a gifted and intellectual boy, proof against every temptation to vice and to amusement; lives five years with a patriarchal apothecary, learning from him not only all manner of medical and surgical knowledge, but likewise Latin, French, Drawing, Natural Philosophy, English composition, nay sometimes Greek and German too; and then walks out of the shop to sweat himself into condition for his trials by running over the public courses mentioned above. Of this glowing picture nothing farther need be said, than that we shall be delighted to see it realized, and that when we do, we shall willingly surrender to the Society of Apothecaries the whole medical education of England and Scotland also.

And are we to be told, in the present day, of the advantages, nay, of the necessity, of apprenticeships? When many of the longest heads in the country are convinced of their uselessness, even for the handicraft trades, will the Legislature encourage the error that they are necessary for a liberal profession? We happen to know something more of medical apprenticeships than the Society of Apothecaries chooses to avow; and since such false notions have been insinuated into the public ear, we must state our knowledge plainly.

There is not a situation in which a young man is more likely to reap the full fruits of a well-spent apprenticeship, than in our own city. A surgeon here is, in every essential point relative to medical practice, a London apothecary; and the apprentices of both are therefore so far on the same footing. But an Edinburgh surgeon is of higher rank in society, of more general knowledge and accomplishments, and of more liberal understanding. His apprentices, from the very beginning of their service, have always full permission, and are even encouraged by him, to avail themselves freely of the unexampled facilities of instruction which lie at their command in every branch of learning, professional and collateral. And moreover, on account of the inferior extent of the city, they are always under the eye of their natural guardians. With these great advantages, such as the English apothecary can never hold out, we can give our opinion sincerely, that Edinburgh apprenticeships do not answer their object. That such is the case, is shown by a fact, which can be proved by individuals of the highest authority,—namely, that apprentices, when they enter on their trials, are found not to be on a par with those who have studied under proper directors, without serving an apprenticeship. And if the reason is sought for, it will be found, as we state from personal knowledge, that apprentices must spend a great deal more time on mere manual operations than is required for mastering them; that their leisure hours for study are cut up and frittered away by frequent calls for their occasional service; that amusement being always at their elbow in the shape of merry companions, it cannot possibly be resisted; that a single idle lad taints the whole shop with idleness. Taking all these things into consideration, and passing over other cooperating causes of less general influence, it is not to be wondered at, that they often regret afterwards their having been bound to masters at all, that they have been known to evade their indentures in order to study at their own hand, and that they will not stand comparison with their companions who have studied so from the beginning under proper conductors. One other important reason for the superiority of the latter must never be lost sight of,—that, besides having their time entirely at their own or their conductor's disposal, they very seldom begin their strict professional studies till later in life, and thus spend one, two, or three years more in the invaluable pursuit of general knowledge.

The only question then is, whether the manual operations acquired during apprenticeships can be learned otherwise. And here we are sure we speak the opinion of every candid man in the profession, when we say, that all, and more than all, the manual skill in pharmacy acquired during apprenticeships may be got by a course of practical chemistry, and two or three months' attendance at the shop of an hospital or of a chemist and druggist; and that the little knowledge picked up of pathological dissections, operations and surgical instruments, will be got far better in one or other of the numerous offices attached to our charitable institutions. It is thus that our graduates and young surgeons, not apprentices, learn the manual operations. Nor are they found less dexterous at them than those who are qualified by a regularly discharged indenture.

But we cry the London Apothecary mercy. There is something else still which he teaches, and which is not taught by the system alluded to, nor in Edinburgh at all, nor any where whatsoever except in an English Apothecary's shop. Their profession is styled in the bill an art and mystery. In what the mystery of the art consists may be guessed at from the following fact:—a gentleman of our acquaintance served a regular apprenticeship under one of the first surgeons in this city,—spent some time as pupil to a well-conducted dispensary,—acted six months as dresser in our Infirmary,—passed his trials for a medical degree and surgeon's diploma after the fullest possible course of study,—lived nearly three years, first as physicians' and then as surgeon's assistant, in our Fever Hospital and the Royal Infirmary, (places of medical education, which we have good reason to believe, are unrivalled by any other in Europe for practical instruction of every kind),—and while he enjoyed all these opportunities, neglected none of them: Nevertheless, on subsequently going to the metropolis, he found he had still to learn the art of prescribing—according to the fashion of an English apothecary. He had to learn, according to Mr Alcock's system, "to renew labels and make indexes," to put a dozen ingredients in a mixture when two would serve, to make a pennyworth of drugs look, taste, and smell as if it was worth the half crown charged for it. This, to be sure, cannot be helped. The English Apothecary's code of customs is as unapproachable, where his profit is involved, as the laws of the English realm. His fee must continue to come to him through the old muddy channel of mixtures and clysters. We shall inquire by and by what public or private good is served by this mysterious part of their trade. Meanwhile, we will venture to say that, mystery though it be, a diligent student may learn it all in three months; that he will strip the apothecary's tree of knowledge of its whole fruit in double the time; and that all he learns after that is merely "*resaignare, repurgare, re-clysterem dare.*"

But it is not merely an apprenticeship, it is one of five year's duration that the apothecaries insist upon. It is really impossible to look at this part of their bill with common patience. Can any thing be more preposterous than that a set of men struggling, by their own avowal, for gentility, should insist on their youth living for five entire years in a world of bottles and galli-pots? Can there be conceived a greater outrage to reason, than that the Legislature should compel the students of a liberal profession to spend the five best years of their life in a shop?

It must be understood, however, that we are far from decrying apprenticeships altogether. There is a description of students who will never act so as to deserve the name, unless under a compulsory system. There are a few masters here, and probably in London too, whose apprentices will be sure to do them credit at all times. But the apothecaries' orator, Mr Alcock, must not on these accounts accuse us of "directing our weapons against the abuse of, and not against the benefits to be derived from, apprenticeships;"—not a bad specimen, be it observed in passing, of the Universal apothecary's lessons to his apprentice in English composition. We have said, and we can prove it, that the benefits of an apprenticeship may be all had in a much shorter time elsewhere. And if we have brought its abuses forth into view, we have likewise shown that they form an integrant and inseparable, and large part too of the system. But we do not for all that wish to exterminate the system. We do not intend petitioning Parliament to forbid apothecaries' apprentices practising in England. We are quite willing to allow Mr Alcock to communicate "all the benefits to be derived from apprenticeships," provided he will not ask the Legislature to arm his apprentices with the law against their betters.

With regard to the other rule to be observed in rearing a thorough-bred apothecary, namely, the giving him "a sufficient medical education," we have very little to say about it. We merely request the reader to attend to the double face which the Society puts on in the matter. For, on the one hand, the bill says, candidates shall serve a five years' apprenticeship, *and* get a sufficient medical education; thereby plainly intimating what we have been striving for, that serving an apprenticeship is not a medical education. And on the other its organ, Mr Alcock, declares that an apprenticeship is every thing, and all but avows that the Society's "sufficient medical education" is, as we well believe it, a mere jumble and humbug. The plain inference must be, that an apprenticeship is not education, and that the "sufficient education" of the Society is naught.

Let us now, however, leave the apothecaries for a moment, and contrast with their system of tuition that pursued by the other bodies whose licentiates they would exclude from English practice. The corporate bodies whom they are to drive away are the Universities of Edinburgh, Glasgow, and Dublin, and the surgical Colleges of London, Edinburgh and Dublin. As it would lead us too much into detail were we to take up the cause of all these schools, we shall single out those of Edinburgh as being the objects of deepest interest both to us and to the party most concerned, the country districts of England.

The medical degree of Edinburgh requires four years of medical and surgical study, which, as they are passed at our University, constitute, on a fair average, one session of six months and three of nine. The subjects of imperative study are Anatomy and Surgery,—Chemistry and Pharmacy,—Practice of Physic,—Theory of Medicine or Physiology, Pathology and Therapeutics,—Materia Medica and Pharmacy,—Clinical Medicine,—Midwifery and the diseases of women and children (each for six months)—Botany for three months,—and an Hospital for eighteen months; to which the student must add any two of the five following—Practical Anatomy, Clinical Surgery, Medical Jurisprudence and Police, Military Surgery, and Natural History;—each three months' courses. The last rule is intended to enhance the candidates qualifications, and to allow him at the same time to suit them to his particular destination. Besides these statutory studies, many candidates attend courses of Practical Chemistry, frequent the Lying-in Institution, and serve as dressers in the Infirmary, or as physicians' or surgeons' assistants; and all who are not apprentices learn the compounding of medicines in the shops of surgeons or druggists. Lastly, they undergo an examination chiefly in Latin; which, whatever some of our maligners may presume to say, is such as few of them would like to risk, and such as sometimes throws back a fifth part of the candidates. Over and above all this, some of them take advantage of the opportunities they possess, of studying the collateral sciences, and many of them, before settling, repair to London, Paris, and Germany, and study for a season in one or more or all of these schools. Will any man presume to deny, that a system like this is calculated to make a general practitioner, such as the plan laid down in the Apothecaries' Act can never form?

But this is not yet all. A very great proportion of our graduates who settle in England, as they are intended for the most part for general practice, likewise study surgery out of the walls of the University; and such students do generally add to their degree the diploma of the College of Surgeons of London or Edinburgh.

The qualifications required of candidates for a Surgeon's Diploma are a fair knowledge of Latin, three years study, attendance on six months' courses of Lectures on Anatomy—Chemistry—Practice of Physic—Theory of Physic—Surgery—Clinical Surgery—Materia Medica—Midwifery—and twelve months of an hospital—all under Professors of Universities, or members of one of the three Colleges of Surgeons. Their studies, like those of the graduates, are closed with an examination, not harsh or capricious, but so severe therewithal, that of 428 applicants during four years previous to last July, more than a seventh part were remitted to their studies. This diploma has been long welcomed in the country districts of England as designating the trustworthy general practitioner. What does the Apothecaries' Society think of it? Would not even Mr Alcock, if his life was in peril, prefer an Edinburgh graduate or surgeon to his own beau ideal of a London apprentice?

And, by the way, this reminds us in good time of a very important fact, which at once puts apprenticeships in the right point of view, and which the Legislature, if it is not too late, would do well to weigh carefully. After the gradual withdrawing of various reservations in behalf of apprentices, the last code of study of the Edinburgh College of Surgeons contains not a single clause in their favour. May we be permitted to explain for them the reasons for the change? They simply found, that although apprenticeships were very good things sometimes, all their advantages might be drawn from other quarters; they found on their trial-days, that those who were not apprentices, had repaired to other sources of information; and they saw, that their plan of examination secured the requisite knowledge, whatever might be the way it was come by. Instead, therefore, of compelling each candidate to serve the customers, and fill the purses of its Fellows, the College does not even tempt him to do so by a single boon. The apothecaries' association was, we fear, all in a buzz, when we told them not long ago, that an Edinburgh surgeon was a man of more liberal mind and endowment than a London apothecary. What do they think of it now themselves?

Let us next look at the state of the counties of England as to general practice, and the quarters from which it has been for some time supplied with general practitioners.

The Associated Society of Apothecaries consisted, in 1822, of about 1500 members, of whom fully 200 belonged to London. There were, therefore, in the counties of England, seven years after the act came into force, 1300 general practitioners, who were genuine apothecaries. It may be easily conceived, that this number did not include the whole general practitioners; in the first place, because it would give a much larger proportion of inhabitants to each than the actual *ratio;* and, secondly, because it may be taken for granted, that a great number of mere shopmen and druggists would join the association at its birth, partly to get a name, partly to escape the sweep of its reforming hand.

It is impossible for us to conjecture the probable amount of general practitioners not apothecaries; but a tolerably correct estimate may be formed of the number of Edinburgh graduates who settle in England. The graduations during the last three years have been 343, of whom 124 were from England and Wales. If we abstract from this number a proportion for those who practise as pure physicians, and for those who enter the service of the army, navy, and colonies, there will still remain at a low estimate 64, or 21 annually sent back to practice in their native country as general practitioners. So that, including the Scotchmen, Irishmen, and natives of the colonies, who also settle in England, we may fairly reckon that the English counties receive from 25 to 30 general practitioners from our University every year.

The number of surgeons who leave this for England with the diploma of our College of Surgeons is not considerable, and probably most of them have also the degree of Doctor of Medicine. The proportion of our graduates, who likewise take with them the diploma of our College of Surgeons, is increasing every year.

So much for the extent of the disqualification which it is intended to establish by the Apothecaries' bill. It may be right, too, to give a little insight into the relative professional character of the persons disqualified. If, to convey to the general reader (whom we wish never to lose sight of at present) a correct notion of the professional spirit imbibed by an Edinburgh graduate or surgeon, was as easy as to put him up to the mystery of the art of an English apothecary, we should attempt it with pleasure. It can hardly be done, however, without greater detail than our limits or the reader's patience would allow.

There is one effect of that spirit which we may notice shortly, as it is easily comprehended, is connected with an important point of pure practice, and therewithal is perfectly notorious to the whole profession. It is the simplicity of their prescriptions and materia medica generally. The Pharmacopeia of Edinburgh is confessedly the simplest in Europe; and yet there has not been a revisal of it for eight years. Its contents, nevertheless, are beyond our exigencies. Of the articles contained in it, those actually in general use do not exceed 50 in number; and even a third of the 50 are of rare application. The prescriptions made up from them are equally simple. They seldom contain more than two active ingredients, often only one, and the vehicle is always the plainest possible. There are still among us, it is true, one or two straggling followers of the old school, who stick to the old plan of torturing the forms of their drugs and the palates of their patients. But such whims have at length almost passed away. No one now fears that, in his patient's eye, a drug will not seem a remedy, unless its very look and smell throw his countenance into convulsions. No one now conceives that diseases abhor complex mixtures and nauseous tastes. And an Edinburgh practitioner will no longer hesitate to give his medicine in mere water, if it will serve, or to please his patient's palate, if he can.

We need not stop to contrast the present system of Edinburgh prescriptions with that followed by the English apothecaries; because they themselves acknowledge and lament the complexity of their medicines, and the quantity they say they must flood their patients with. The late manifestos in their Transactions and Parliamentary bills repeatedly take notice of this evil; so that all we have to add is merely, that their system is not only a sign, but in so far likewise a cause of the low state of English physic, compared with that of the Edinburgh schools.

It is an odd fancy of theirs, however, that the evil cannot be remedied without Parliamentary interference. In their first bill, they had a clause empowering them to take visit-fees, and to prosecute for them "in such manner as they now sue for and recover their charges for medicine." And the avowed object of this clause, as may be gathered from their Proclamation (*Trans.* p. 121, *et seq.*), was to aid in raising their gentility to a level with that of the physician and surgeon, and to take away the obvious temptation under which they lie to keep up a reciprocal current of drugs and gold between their shop and their patient. Nobody can deny that both objects were very laudable. But it may be doubted whether they took the right road to them.

They would simplify their medicines, and lessen the distribution of them. Can they not do so without act of Parliament? If their patients must pay them for their attendance by a drug-tax, is there any law to prevent them from levying the tax by the value instead of the bulk of the commodity, or from making the value follow the inverse instead of the direct ratio of its complexness? When an apothecary is going to send his patient three doses of a mixture for the day, each in its bottle, and each with its charge, would either law or reason prohibit him from mixing the whole three in one, and charging three prices for the additional trouble? When all the world knows that his stock of Aqua Fontana is the chief source of his wealth, that the quantity served out represents by tacit agreement the extent of his dealings with his patient, why should he not frankly charge his fee upon it in its pure instead of its adulterated state?

But they would also attain gentility. And we are willing to help them; for truly they seem very ignorant how to go about it. Can the Association be so little acquainted with the rank they aim at, as not to know that a gentleman-practitioner cannot scramble for his fees in a law-court without losing caste? It is a hard custom certainly on particular occasions. But none of the customs agreed on by society for marking out the gentleman is without its occasional hardships. So rigorously is it followed in this part of the country, that there have been only one or two violations of it among us for a long time; and these exceptions merely confirm the rule: There has been something so singularly forbidding either in the nature or in the fate of the causes hitherto tried, that their number is by no means likely to be augmented.

The Association will now comprehend what is required for reforming their practice and raising their gentility. Let it be understood among them generally, that they are to charge less for their medicines. Their patients will then see the necessity of remunerating them in fees, as is often actually practised already; or if they should not, where would be the harm in the Association pointing it out? A plan of this kind would effect a reform gradually; and it is only a gradual reform that a reasonable apothecary would look for. It would certainly still leave room for "unqualified men and midwives usurping their character;" and it would likewise cost them some fees on account of niggardly patients. But the usurpers will never be able to assume more than the vulgar part of their character, and can never be prevented from doing so by acts and lawsuits. And as to the occasional loss of fees,—that is the price they must pay for gentility. It would seldom be considerable, however, in the case of a gentleman apothecary: and if an apothecary's practice is of a kind that he cannot realize his income without a legal right to prosecute for it, the darling object of the Association, gentility, is in his case altogether out of the question.

There is another way in which the reader might be shown the relative qualifications of practitioners sent to England from our schools and from the apothecaries' shops,—namely, the celebrity acquired by each in the scientific world, and in the field of practice. But we are here met by a difficulty at the very outset. Fame does not supply us with the name of a single apothecary for the contrast.

It is well known, that the practitioners formed in our schools are held in the highest estimation over the whole world. In every rank of the profession, they will stand comparison, to say the least, with those of similar rank educated in England. If we look to past times, there never has been a physician of eminence in London, but there has been at the same time one of at least equal eminence in Edinburgh. Now that Baillie has disappeared, the comparison still holds good. In pure surgery we are at length treading close on the heels of our metropolitan brethren. In general or family practitioners we are still far a-head of them, if we abstract that numerous class in England whose education and spirit constitute them properly part of ourselves. In all the departments of the public service, and in all the regions of private practice, the pupils of our schools are foremost in place and in renown. Nay the number sent out from among us continues to increase gradually and steadily. Last year the graduates of the University were 140 in number, and the medical students 1000; and this in spite of the schemes of its enemies, and the unnatural efforts of one of its own members to undermine it. Is this a source which the Legislature would drain, merely to glut the cravings of 1500 apothecaries?

But perhaps we have all along mistaken the apothecaries' intentions, and done them wrong. Some of our liberal friends among them who have been hoodwinked by the professions of liberality invariably held out by their Committee, will say: This is quite a false alarm: We merely wish the Legislature to legalize our desire for gentility, to aid us in clearing the land of quacks, to give us leave to pound the druggists into apothecaries: We have no hidden plot against your old pupils, whose professional character places them far out of our reach. For only look what our Committee says on the subject: "Actuated by no sinister motives; *seeking no exclusive advantages;* having in view less the interests of the profession than of the community at large, your Committee begs leave to express its decided opinion, that this Association is fairly entitled to the firm support and zealous co-operation not only of its own members, but of every enlightened medical practitioner, and to the countenance and good wishes of all feeling and virtuous men." (*Report, July* 3, 1822.) "Many laws act powerfully, though silently, as means of prevention, although no case occurs which calls for their interference. The penal acts against Dissenters form an instance of this. They seem to be a dead letter; because, for many years at least, they have not been called into operation; but their existence has prevented such acts of individuals occurring as they rendered illegal." (*Introd. Essay, Trans.* p. lvii.) It is quite clear, that our Committee abhors monopoly, and respects Scottish graduates and surgeons; that their wrath is to be turned against quacks only; and that, as to practitioners coming from your schools, our Act is to serve at the utmost as a mere bugbear for frightening those who may at times slip through your fingers unqualified. And what is the state of the fact? Our Committee has not prosecuted one of your people; and our countrymen are not afraid they ever will; for, according to your own confession, they are repairing to your University and surgical school in greater crowds every year.

To such language we answer:—We are not to be deceived by the Society's professions. We do not like the keenness with which the District-committees have been slaying and devouring quacks in some counties; we fear that this food will fail before their appetite is appeased, and that they will then look round for better. We do not like the hearty commendations bestowed on their ravenous spirit by the General Committee. We do not relish the spirit breathed by the General Committee itself; as, for example, when it tells us, that "although no conviction, under the Apothecaries' act, may have yet taken place of any one above the rank of the most common bone-setter,—the fear of prosecution will prevent men from engaging in practice *in defiance of its provisions*." We are quite aware that other bodies, possessed of ancient prerogatives, have been moderate in exercising them, and have waived them as obsolete. We know that the Edinburgh College of Surgeons permit persons, not their Fellows, to get into good practice in this city, without molesting them. But we cannot give a mushroom corporation—we cannot give the Apothecaries' Society credit for such forbearance. We do not comprehend the meaning of a new act which is to be a dead letter; or why the Association should have been at so much pains to bring forth a bantling, which they intended should be dead-born. We strongly suspect it is only feigning dead, lest public opinion should stifle it in its infancy; and that we shall see it rise up in life and spirit against us, as soon as it has sucked a little strength from the blood of the poor quacks and bone-setters.

We have better reasons, too, for dreading all this, than a timorous interpretation of the Committee's reports. For they have actually shown their disposition to try their strength upon a higher order of interlopers. There may not have been an actual "conviction under the act of any person above the rank of the most ignorant bone-setter." But already, on more occasions than one, their District-committees have tried to drive away practitioners licensed by the Edinburgh College of Surgeons. The General Committee know very well, that the Edinburgh College has been repeatedly addressed by its licentiates in England, complaining of acts of oppression on the part of the Apothecaries' Society, and that the College has been obliged to content itself with lamenting its inability to give relief. But as the Committee's constituents may not be aware of this, and may well feel indignant at such proceedings on the part of their representatives, we shall state one fact by way of example. A gentleman, after receiving his diploma from the Edinburgh College of Surgeons, pursuing other studies at the University, and living some time as house-surgeon in a considerable county hospital, settled in Colchester. For some time he was left in quiet possession—of an unproductive practice. But at length he began to cut rather deep on an apothecary of the place. And, accordingly, last summer, two years after he settled, he was first threatened, and then summoned to show by what right he practised. The Society's case broke down, however, for what reason it is not necessary to add; but it was neither because the Society admitted his actual qualification, nor because he had an Edinburgh diploma.

With deference to the Committee, therefore, we desire some better guarantee for our security than their professions. And since, if these professions are real, it can make no odds to them, while it would bring back our peace of mind, we shall reckon it a favour if they will introduce a clause in behalf of at least Edinburgh graduates and surgeons, similar to that granted in behalf of surgeons in the Army, Navy, and East India Company's service. The character of first medical school in the empire, a character which we still keep unsullied, first got us a share in the supply of English practitioners; long possession and upright management have given us a solid title to that share; and, if the medical practice of England is to be partitioned by act of Parliament, we must lay claim to a chartered right in our actual property.

We say—if it is to be partitioned; because, although we have not discussed it, the question of Parliamentary interference at all is by no means so clear as the Apothecaries' Committee would make it. And this much may be said in conclusion,—that if Parliament will legislate for the medical profession, it must now be aware from the arts of the apothecaries,—from the growing dissensions of the London College of Surgeons, and their fruitless efforts to extend their charter,—from the illiberality and groundless pretensions of the London College of Physicians, that without glaring injustice, it is quite impossible to legislate unless for all departments of the profession at once, and for all the three kingdoms together. We know not what our Southern friends may think of such a proposal. But it is perfectly disinterested on our part, for neither of our Colleges has any thing to gain by it, and at the same time they are quite ready to submit to scrutiny and reform.

X.

Medical Essays. Essay First. On the Effects of Intestinal Irritation. Essay Second. On some Effects of Loss of Blood. Essay Third. On Exhaustion and Sinking from various causes. By MARSHALL HALL, M.D. F.R.S.E. Physician to the General Hospital near Nottingham, &c. &c. London. Printed for Longman, Hurst, Rees, Orme and Brown, Paternoster-row. 1825. pp. 96.

THIS small volume contains some things which, if not absolute novelties, are at least entitled to the character of giving new views of subjects already familiar. If the practitioner is summoned to a case in which he recognises, or thinks he recognises, symptoms of *enteritis*, he is not to use the lancet incontinently and precipitately; he is to pause and deliberate with himself whether this measure may be simply useless, or whether it may not prove to be highly prejudicial. The disease may be not intestinal inflammation, but intestinal irritation only. Such a patient may be blooded until scarcely a drop remains in his veins, not only without relieving the symptoms, but with the effect of inducing others more serious. He may have severe headach, intolerance of light and sound, throbbing temples, a flushed countenance and general heat; his senses may wander, his intellects may waver; his pulse may be quick, his tongue foul, his thirst great; his hours of sleep may be converted into incessant and exhausting restlessness, or into short hurried slumbers, disturbed with frightful dreams, or alternating with hideous waking visions; but the evacuating system would, in such a case, either prolong and aggravate all his sufferings, or plunge him by a precipitate step into the grave. If this untoward event does not snap the chain asunder abruptly, another order of complaints may appear, to which the character of pleuritic or thoracic inflammation is allotted; and in this circle incessant changes will be rung, until the disorder has traversed every region and organ of the human body.

Again, if the advice of the medical attendant is requested to remove various ailments generally referred to the head, which may take place in the course of treatment for any disease, medical or surgical, or in the progress of convalescence, he must by no means, and on no account, treat them on the notion that they arise from inflammation or plethora of the vessels of the brain. After some sensations of chilliness, the patient may have headach, heat of the face and eyes, throbbing of the temples, general heat of the skin, and perhaps some wandering pains in the chest, belly, back, or limbs. Yet with a pulse at 100 or more, a vein is not to be rashly and immediately opened; for this, though it might relieve some of the complaints, and render the pulse less strong, if it did not render it less frequent, would not operate on the primary seat of the disease, and consequently would fail in effecting its final removal. The complaints under which the patient is labouring is neither fever, phrenzy, local inflammation, nor rheumatism; and is not to be treated as either of these disorders are. It is merely intestinal irritation.

Further, in disorders truly inflammatory, and which cannot be removed without bloodletting, this remedy may be carried to an undue and injurious extent. Whether it is sufficient to subdue the original disease or not, a sudden and copious evacuation of blood from the veins may be followed by alarming and mortal weakness, or by the supervention of symptoms of disorder of the head, and disturbance of the functions of sense and intellect, so great as not merely to perplex the medical attendant, but threaten the life of the patient.

Lastly, a similar state of extreme weakness and languor may supervene in various diseases, of infancy, of manhood, or of old age; and where it was neither dreaded nor expected, a state of slow but certain failure of all the powers of the system and all the functions connected with them, may terminate gradually in apparent suspension of sensation, from which there is no recovery.

Is this a faithful and true delineation? Are these several forms of disorder, with the causes from which they are alleged to spring, actual existences, and not the creations of fancy? Is their truth established on the strength of facts too well authenticated to be doubted, too numerous to be regarded as anomalies or exceptions, and too simple to be the result of partial and mistaken observation? Are we to suppose that the characteristic distinctions of diseases have been so long unknown, so little understood, or so much overlooked as to be treated with improper or injurious measures; or is it to be imagined that the necessary limits within which the use of these remedies ought to be restricted, were so imperfectly defined, and so erroneously applied as to convert them into causes of disease?

Though these questions are naturally suggested by the first observations which occur in the Tract now before us, it is by no means easy to give them an immediate or satisfactory answer. By different individuals, also, they will be answered in different modes; some being disposed, we doubt not, to deny entirely the views given by Dr Hall; others though willing to admit them, yet unable to see on what grounds they are to be explained; and some perhaps seeing in them nothing but the ignorance and carelessness of the observer, and the injudicious employment of the means of cure. How far either of these modes of viewing the subjects, to which Dr M. Hall has requested the attention of his professional brethren, may be correct, we know not, and must acknowledge our unwillingness to deliver a decided opinion; but that our readers may form some idea of the grounds on which Dr Hall has formed his conclusions, we shall proceed to give a short view of the three Essays which compose the present publication.

Intestinal irritation, the subject of the first Essay, is represented by Dr Hall as a disease hitherto undescribed in medical writings, and by no means understood in practice, or distinguished from other morbid affections of a very different nature. In many instances it appears with symptoms of the most acute *phrenitis;* in others, with those of intestinal or peritoneal inflammation; occasionally it assumes the symptoms of pleurisy; and accompanied with palpitation, it may suggest the idea of disease or acute inflammation of the heart. The general symptoms are said to be,—shivering more distinct than in cases of inflammation; afterwards heat of surface, with disorder of the head, chest, or belly, or of all together. There is headach, giddiness, and some morbid impression on the mind; panting and fluttering about the heart, with general hurry, irritability, and restlessness; the tongue is white and furred; the alvine evacuations are dark-coloured, fœtid, and scybalous, or yellow like yolk of egg, or of the appearance of yeast; and the urine is said to be turbid, and frequently to deposit a copious sediment.

The more particular assemblage and distribution of symptoms may be stated in the words of the author.

"The affection of the *Head* consists of the most acute pain, the greatest intolerance of light and sound, and the severest form of vertigo, wakefulness, and distress, and sometimes even delirium, and the pupils of the eyes are often extremely contracted.

"The affection of the *Chest* is denoted by severe and acute pain of some part, which is apt to vary its situation, passing from one side to the other, or to the *back*, or occupying a situation higher up or lower down; this pain checks a deep inspiration, and even the ordinary breathing, to which it imparts a character of difficulty and anxiety.

"When the *Abdomen* is affected, there are acute pain, and great tenderness under pressure, in some part, or more or less generally diffused. The attack and situation of the pain is such, in some instances, that the case is with difficulty distinguished from *gall-stones*, though it more generally resembles *enteritis*.

" When the *Heart* is the seat of this affection, there are violent and terrific attacks of palpitation,—and the course of the carotids, and even of the abdominal aorta, is sometimes the seat of violent pulsation or throbbing."

The foregoing description is however insufficient to establish a well-marked difference between the symptoms which arise from inflammation or congestion in the head, chest or belly, and the effects to which Dr Marshall Hall alleges intestinal irritation may give rise in these several regions of the body; and it is manifest, that it would be extremely difficult to draw the line of distinction in any given case between an attack of phrensy, of pueumonic, pleuritic, or cardiac inflammation, and of peritoneal or enteric inflammation, and this disorder, alleged to be of a very opposite nature, and ascribed by the author to intestinal irritation. With the view of removing this difficulty, Dr Marshall Hall draws a very elaborate diagnosis, the following extracts from which it is absolutely necessary that our readers study with attention.

" The affection of the *Head* from intestinal irritation comes on suddenly, is formed all at once, and is attended by great restlessness, suffering, and distress. In *phrenitis*, the disease is usually formed somewhat more gradually; the patient has been subject to pain of the head perhaps for some days or even longer; he complains less; or at least there is less urgent distress,—less distress of a general kind; the pain may be very severe, although it is more frequently rather obscure; the intolerance of light and sound is less urgent; the rigor, and subsequent heat, and the attack in general are less marked; the patient is not so soon *relieved* by remedies, and the tongue and alvine evacuations are less morbid. In the attack of affection of the head from intestinal irritation, the patient is *relieved* perhaps completely if the lancet be employed, but the attack soon recurs with equal or greater violence; in phrenitis, the relief is seldom so complete, the interval of ease so long, or the return so marked,—the pain is *diminished*, perhaps, but gradually resumes its former violence, unless active measures be interposed.

" When the *Chest* is affected from intestinal irritation, the pain is severe and acute, and increased by a full inspiration. If the inspiration be repeated, however, a second and a third time, the increase of the pain is less and less; the situation of the pain varies; there is no cough,—and no crepitus on making a full expiration. In all these respects the case differs from inflammation. The remarks already made respecting the relief from remedies, the tendency to a sudden recurrence of the pain, &c. in cases of affection of the head, apply equally here.

" I had long remarked that there might be both acute pain and tenderness under pressure of the abdomen, without inflammation. This state of things is frequently the result of intestinal irritation. It is distinguished from inflammation by the *general* symptoms of this affection,—the mode of attack,—the effects of remedies. In inflammation, the surface is usually cool, the head unaffected, the patient remarkably quiet. In the case of intestinal irritation, on the contrary, there is generally much heat after rigor, the head is much affected, and the patient is restless and generally distressed, the tongue is loaded and perhaps swollen, the alvine evacuations are extremely morbid, and great relief is obtained by the free operation of medicine."

We know not whether these extracts will enable our readers to form any idea of the views of Dr M. Hall, or to perceive what this physician understands by the disorder to which he applies the name of intestinal irritation. If they do not, we must refer them to the cases adduced by the ingenious author in support of his opinions; and request them to compare these cases with such as have fallen within the range of their own observation.

The first of these cases, which did not occur in the practice of the author, is one in which the medical attendant took the symptoms successively for those of *enteritis*, inflammation of the brain, abdominal inflammation, and lastly pleurisy, and treated them in each of these forms by the usual depleting remedies,—always with the effect of controlling their violence and effecting their removal. This case, Dr Marshall Hall thinks he has reason to regard as an instance of "intestinal irritation, and of its effects on several organs in succession." But where is the proof of this, it may naturally be asked? Does the case itself suggest any circumstances which must be admitted as unequivocal evidence of the correctness of this opinion? It does not; and the only reason which the ingenious author assigns for his hypothesis of its pathology, for to this name only is the opinion of Dr Hall entitled, is founded on the phenomena and results of the other cases which he records. Upon looking somewhat narrowly into these, we think we have some reason to offer certain observations on them.

The first of them, which is the second in the list, is one which the author himself admits to have presented the appearance of peritoneal inflammation. The symptoms were so severe as apparently to demand the repeated employment of the lancet, and the application of leeches, so that the patient lost about thirty-five or forty ounces of blood; and the bowels were freely moved, when it is said "the stools were very fetid." All the symptoms were removed on the third day, and every thing promised a speedy and secure convalescence. Early on the succeeding day, however, Dr Hall received an urgent request to see his patient.

"She had been seized with severe pain of the head, especially over the eye-brows, attended by beating and throbbing, and by the most urgent intolerance of light—so that the eyes could not be opened for a moment for examination; the pain was increased on attempting to sit up erect; the countenance was palish and sallow; the pulse full and frequent; there was no faintness or sighing." pp. 9–10.

A draught with thirty drops of laudanum and of the aromatic spirit of hartshorn was prescribed, and in the course of an hour and a half the patient who had enjoyed some comfortable sleep, was able to bear the light, suffering much less pain, and in every respect better. The bowels were then freely emptied, light nourishment was given, and the anodyne aromatic draught was administered, while a cold lotion was applied to the head. Under this management the recovery was progressive and uninterrupted.

Dr Marshall Hall has the candour to acknowledge, that he now thinks this case was at first mistaken for *enteritis*, and that the treatment by the lancet and local bleeding was unnecessary; and he expresses great astonishment that the symptoms of the second or head attack, simulating phrenitis, were removed without the lancet by the exhibition of an ammoniacal anodyne draught. We, in our turn, must be excused if we express our astonishment that it should have been otherwise; and we see nothing more in this case of peritoneal inflammation, termed by Dr Hall *intestinal irritation*, than what we have seen in many other inflammatory diseases after copious detraction of blood. It so happens, that we can adduce, besides others, a case so completely in point, that we cannot refrain from mentioning the facts of it very shortly. In June 1822, when peritoneal inflammation was still prevailing in this city, we were requested to see a stout plethoric woman, who had been delivered the day before by a midwife of a healthy child. When we reached the house, the woman was still in the rigor or fit of cold shivering with which she had been attacked, and by the occurrence of which she had been much alarmed. She complained of no local uneasiness, unless what resulted from the sensation of cold. An opiate was given, warmth was applied to the extremities, and to the pit of the stomach, with the apparent effect of alleviating and shortening the shivering fit; and she was ordered to take half an ounce of castor oil as soon as the heat of the system was re-established, which took place about twelve at noon. In the afternoon, she had pain of the belly, most uneasy and severe in the umbilical and hypogastric regions, aggravated by coughing and pressure, and with a good deal of tension. The skin was hot, the tongue furred, the thirst was intense, the temples throbbed with headach, and the pulse was 122, and sharp. Forty ounces of blood were drawn from the arm quickly, and in a full stream, which induced first paleness, and then very transitory fainting, and a draught was given her with forty drops of laudanum. The pulse had not been rendered much slower. The following day (June 30th, 1822), after sound sleep, and the bowels being freely opened, the pain of the belly was much relieved, but the pulse was still very quick; and in the afternoon of that day, though she had no pain since morning, coughed and bore abdominal pressure in every point without uneasiness, yet the pulse was 120. The only favourable signs were in the skin being cooler, the tongue more moist, the thirst less, and the headach entirely gone. During the night, she was disturbed by some noisy neighbours about eleven; and the effect of this was a violent and complete fit of spectral delirium. She raved, complained of frightful and threatening figures; and, after about an hour or two of incessant restlessness, she fell into a disturbed uneasy sleep, from which she awoke in the morning with a hot skin, flushed face, and a wild look, furred tongue, immoderate thirst, not a little headach, some remains of delirium, and a pulse at 120. As she had no abdominal pain, however, the lancet was never thought of; the quantity of bed-clothes was diminished, cold was applied to the head, a draught containing fifty drops of laudanum was administered, with the effect of rendering her much calmer, and inducing steady and refreshing sleep. At noon, when she awoke, the pulse was down to 104, and in the evening to 96, and no trace either of delirium or any other complaint of the head remained. Her convalescence from this was regular and uninterrupted.

Of cases like this, it is not easy to explain the theory; but they are so common, and so well understood in practice, that we always imagined the treatment to be well established. The copious blood-letting is sufficient to empty the large vessels, probably many of the small ones, and to act on the system at large; but it neither renders the contractions of the heart less frequent and numerous, nor produces that effect upon the secreting and excreting capillaries, which is necessary to the establishment of the healthy state of the circulation. By some it has been said, that this imperfect and partial effect of blood-letting consists in local congestion; and they accordingly ascribe all the alarming sensations which take place in the head, and in the organs of sensation and intellect, to congestion of the venous system of the brain. It is possible, that something of this kind may take place; but, as drawing blood from the venous system seems a paradoxical method of producing fulness of that system, there must be some other circumstance or condition of the capillary circulation particularly, of which we are not aware, and which we do not perfectly understand. It would lead into too extensive a field of discussion to enter more largely on this topic; and we must be satisfied with referring our readers to what we have said in another article on the subject of *delirium tremens*, to which we are disposed to think the condition of the system alluded to by Dr Marshall Hall bears a close resemblance. That condition consists not in intestinal irritation, but in irritation of the capillary system.

In the third case given by our author, that of Mr Oldknow of Nottingham, we see nothing very singular or difficult to explain. The symptoms which this gentleman began to suffer on the sixth day, after the operation which he had undergone, and which are very distinctly and satisfactorily narrated in his own language, are neither more nor less than the constitutional disorder arising from the state of the wound, and its approach to suppuration. They were somewhat tardy, we admit; but there is so much variation in the time at which such symptoms begin to be fully formed, in the shape which they assume, and in the severity which they attain, that their peculiar appearance in the case of Mr Oldknow, ought to be the source neither of wonder nor of difficulty to any one who is moderately conversant with such cases. The treatment throughout was highly proper, not only by the patient himself, or his surgeon, but by Dr M. Hall afterwards; but had they changed situations with each other—had Dr Hall's "nourishment, volatile salt, and copious enema" been administered at the time when the blood-letting and evacuating system was adopted, either the cure would have been more tedious than it was, or such a termination might have occurred, as would have rendered nourishment, volatile salt, and all the pomp and circumstance of prescription unavailing. It was the previous depletion which gave to the remedies of Dr Hall their salutary effects.

Nor does the fourth case merit very particular attention as illustrative of the soundness of the theory of Dr M. Hall. The patient was in the fourth month of pregnancy, and habitually costive; and surely it is almost superfluous to remind any one practically acquainted with the disorders incident to women in this state, of the variable forms which they may assume. The question of the nature of the disorder in the present case lies in a narrow compass. The patient had repeated attacks of headach, throbbing of the temples, pain and tightness of the chest, and hurry of breathing, with occasional palpitation and cough.

The state of the pulse is not mentioned. But for these symptoms she lost blood repeatedly, took aperient medicine, the blue pill, had blisters applied, and took tincture of foxglove; all without effect it is said. Ten days after the first accession of her complaints, Dr M. Hall found her with pain, throbbing and heaviness of the head, contracted pupils, and intolerance of noise or any sort of disturbance, palpitation of the heart, and a sense of mortal faintness; tightness of the chest, oppressed breathing, and a peculiar tracheal cough; some pain in the region of the womb, but without vaginal discharge; foul, dark tongue, and pulse at 120, and the alvine discharges procured by medicine scybalous, and dark-coloured, afterwards yeasty and offensive.

"I was forcibly struck by a general but marked resemblance of this case, to those already given, and to others of the same nature, which I had witnessed:—the depleting already fully adopted and repeated had proved ineffectual in affording relief; the purgatives hitherto given, were, I believed, inefficient. The plan I proposed was to give efficient purgatives,—to restrain their operation by draughts with *tinctura opii* and *spiritus ammoniæ aromaticus*,—to support the strength by means of nourishment given every hour or oftener,—to procure sleep by anodyne *enemata*,—to guard against exertion or fatigue,—noise or disturbance. The recovery was uniformly progressive."

The whole matter then resolves itself into this question. Did the complaints above enumerated arise from the plethoric state incident to pregnancy, or from a disordered condition of the intestinal canal and of the alimentary function, or from both causes taken together? We presume it can require no long deliberation to reply, that this last circumstance was the true origin of the symptoms, which Dr M. Hall here ascribes to intestinal irritation solely and exclusively.

The fifth, sixth, and seventh cases, which complete the list, present nothing peculiarly demanding inquiry or attention; and if the curiosity of our readers be not gratified on this point, we trust they will have patience to peruse the ingenious Essay itself, which is destitute neither of interest nor instruction.

Upon the whole, though we deny not the frequency of disorder of the intestinal canal, and the influence which it exerts in inducing some diseases and modifying the appearance of others, we cannot agree with the opinions of the author of this Essay, in ascribing to it the effects which he alleges to have arisen from this cause in the cases now adduced. We have seen these effects so frequently in many different and well-marked diseases, that we have no hesitation in ascribing them, not to intestinal irritation, but to a peculiar state of the capillary system, arising from great and sudden loss of blood, just before that change has taken place, whatever it may be, which re-establishes the balance and equal distribution of the circulation. This state consists not in inflammation; for there is no proof of the existence of this; but in capillary irritation, if it may be so called. We have seen it after copious blood-letting, for the cure of *pneumonia*, for that of *enteritis*, and for that of rheumatism; we have seen it in a slight and transitory form after blood-letting, carried to a considerable extent for the removal of dropsy; and it is no long time since we saw a very well-marked example of it, where large and repeated venesection had been rendered indispensable for controlling a severe attack of rose of the head and face. Yet this condition, so alarming, we have almost invariably and uniformly seen disappear under the use of large doses of opium.

The subject of the second Essay, on the effects of loss of blood, is not less important, and is if possible more difficult. Dr Marshall Hall is however mistaken when he asserts that it has escaped in a great measure the attention of physiologists, and of the practical physician. Haller has left a valuable memorial of the attention which it has claimed, in a physiological point of view, in his Essay and his anatomical experiments *De Motu Sanguinis.*[*] Frederic Hoffmann, more than a century ago, wrote on the use and abuse of blood-letting; and scarcely a year elapses without some treatise or essay on the pernicious extent to which blood-letting may be carried, as a remedy in the treatment of diseases.

The Essay of Dr M. Hall embraces a pretty wide field of discussion. He *first* inquires into the immediate effects of loss of blood, as fainting, and the ordinary reaction. *Secondly*, he considers the remote or cumulative effects of loss of blood, such as exhaustion with or without reaction, exhaustion with defective reaction, and exhaustion with that peculiar state which is termed by medical observers *sinking*. In the *third* part of his Essay, he takes a view of the effects of further loss in cases where it has already produced exhaustion. In the *fourth* place, he inquires into the influence of age, of disease, of temperament, &c. on the effects of loss of blood; and he concludes his Essay with a short sketch of the effects of loss of blood on the internal organs, as the brain, the heart, the lungs, the intestinal canal, &c. Without attempting to follow our author over this wide and extensive field, we shall confine our present observations to one or two topics only.

The first and most important effect resulting from great loss of blood, which we regard it of importance to remark, is that which is described by Dr Marshall Hall in the following terms, under the denomination of reaction.

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[*] Alberti V. Haller, Opera Minora, Tom. I. Lausannæ, 1762, Caput V. p. 212.

"The phenomena are very different, if, instead of one full bleeding to syncope, or of a profuse hæmorrhagy and even protracted syncope, the person be subjected to repeated blood-lettings or to a continued drain. In this case, within certain limits, the pulse, instead of being slow and feeble, acquires a morbid frequency and a throbbing beat, and there are, in some instances, all the symptoms of *excessive reaction* of the system, which it is my object now to describe.

"The state of excessive reaction is formed gradually, and consists, at first, in forcible beating of the pulse, of the carotids, and of the heart, accompanied by a sense of throbbing in the head, of palpitation of the heart, and eventually perhaps of beating or throbbing in the scrobiculus cordis and in the course of the aorta. This state of reaction is augmented occasionally by a turbulent dream, mental agitation, or bodily exertion. At other times it is modified by a temporary faintness or syncope.

"In the more exquisite cases of excessive reaction the symptoms are still more strongly marked, and demand a fuller description.

"The beating of the temples is at length accompanied by a throbbing pain of the head, and the energies and sensibilities of the *brain* are morbidly augmented; sometimes there is intolerance of light, but still more frequently intolerance of noise and of disturbance of any kind, requiring stillness to be strictly enjoined, the knockers to be tied, and straw to be strewed along the pavement; the sleep is agitated and disturbed by fearful dreams, and the patient is liable to awake or be awoke in a state of great hurry of mind, sometimes almost approaching to delirium; sometimes there is slight delirium, and occasionally even continued delirium; more frequently there are great noises in the head as of singing,—of crackers,—of a storm,—or of a cataract; in some instances there are flashes of light; sometimes there is a sense of great pressure or tightness in one part or round the head, as if the skull were pressed by an iron nail, or bound by an iron hoop.

"The action of the *heart* and *arteries* is morbidly increased, and there are great palpitation, and visible throbbing of the cartoids, and sometimes even of the abdominal aorta,—augmented to a still greater degree, by every cause of hurry of mind or exertion of the body, by sudden noises or hurried dreams or wakings; the patient is often greatly alarmed and impressed with the feeling of approaching dissolution; the state of palpitation and throbbing are apt to be changed, at different times, to a feeling of syncope; the effect of sleep is in some instances very extraordinary—sometimes palpitation, at other times a degree of syncope, or an overwhelming feeling of dissolution; the pulse varies from 100 to 120 or 130, and is attended with a forcible jerk or bounding of the artery.

"The *respiration* is apt to be frequent and hurried, and attended with alternate panting and sighing; the movement of expiration is sometimes obviously and singularly blended with a movement communicated by the beat of the heart; the patient requires the smelling bottle, the fan, and fresh air.

"The skin is sometimes hot; and there are frequently general hurry and restlessness.

"In this state of exhaustion, sudden dissolution has sometimes been the immediate consequence of muscular effort on the part of the patient." pp. 42-44.

This description is so accurate, and on the whole so faithful, that few, we believe, have had occasion to practise their profession for a very short time even, without meeting with some examples which could amply confirm the statements of Dr M. Hall. In support of the general accuracy of the description, two very good cases are given, which may be perused by such of our readers as require this confirmation. In short, the existence of this state after extreme loss of blood must be admitted. There is one difficulty, however, attending it, and that is the doubt which naturally suggests itself as to the true cause on which it depends;—viz. whether it is the mere effect of loss of blood, or of some modified form of the disease for which the blood-letting was considered necessary. It is to be remarked, in the first place, that this disordered state of the system and of the functions does not take place invariably after great and repeated evacuations of blood; secondly, that it may take place without bearing any proportion to the quantity of blood taken, or to the frequency with which it has been taken; and, lastly, that in cases in which recovery does not eventually take place, the remains either of the original disease, or of some similar one, is found to have terminated the existence of the patient.

Thus a man is treated for pneumonia, enteritis, or some other acute disease, by copious venesection, repeated perhaps according to circumstances. The immediate and urgent symptoms are relieved, or disappear; but convalescence does not follow. Though there is neither local pain nor very marked disorder of the functions, the pulse continues quick, the skin dry, the appearance pale, the person weak and emaciated; and all the symptoms of a chronic inflammatory disorder are fully established. Some, it may be said, would regard this as hectic fever; it is so; but hectic fever is in all instances, and without exception, symptomatic; and it remains yet to be shown, what is the line of distinction between the incipient stage of the state of the system to which we now allude, and that to which the author before us applies the name of *reaction*, or *exhaustion with reaction*.

We regard it therefore as exceedingly doubtful what may be the true theory of this state of exhaustion with reaction. Dr Hall thinks it plain, that "the state of sinking involves a greatly impaired state of the functions of all the vital organs, from defective stimulus, and especially of the brain;" and he infers that the respective disorders of the several functions all denote an impaired condition of the nervous energy. He further compares it to the state of the functions in apoplexy, and to the effects observed on abstracting the influence of the brain and spinal marrow, by dividing the eighth pair of nerves, or destroying the lower portion of the latter organ.

When we assert that this is not satisfactory, we believe it will not be requisite to prove that it is liable to this objection. Nor will our readers find much difficulty in admitting that the theory which assigns the same state to the brain during apoplexy, and after its vessels have been unduly drained of blood, is not likely to rest on a very solid foundation.

We have already adverted to the resemblance between this condition of the system and that which takes place in certain forms of inflammation, which we conceive to be chronic. This similarity is admitted by Dr Marshall Hall in his remarks on the effects of the further loss of blood in cases of exhaustion; but he contends that the resemblance is apparent only, and fallacious; and is liable to lead, if practically acted on, to most fatal errors.

"The symptoms of exhaustion with reaction have, I am persuaded, frequently been mistaken for those of *inflammation* or other disease of the *head* or of the *heart*. Under this impression recourse has frequently been had to the further detraction of blood by the lancet. And the effect of this practice is such as greatly to impose upon the inexperienced,—for all the symptoms are perhaps fully relieved.

"It was some time before I could fully comprehend the nature of this fact. I had satisfied myself that, in certain cases, the symptoms were those of loss of blood; and yet it appeared no less certain that those very symptoms were relieved by the lancet. At length I discovered, by a careful observation, that the symptoms which were relieved were those of reaction; and that the mode of relief was by the substitution of syncope; that the relief endured as long as the state of faintishness continued, but returned as this state gave way to the rallying and reaction of the vital powers.

"Another circumstance equally interesting and curious was, that within certain limits, the remedy which relieved for a time, eventually only added to the severity of the malady, for that this was apt to return after a certain period, in a still more aggravated form. It is natural, indeed, to suppose that, unless there was a tendency to the failure of the vital powers, the reaction of the system and the painful circumstances attending it, would be greater after a third or fourth loss of blood, than after a first or second; indeed there are seldom the symptoms of reaction after one flow of blood, however great or profuse; it is the repetition or protraction of the cause which, as I have already observed, is essential to produce this effect." pp. 56–58.

The truth of these observations, and of those on the ultimate effects which may take place if blood continues to be detracted from the system in such a state, is clearly and forcibly illustrated by four cases, in the two last of which repeated and irresistible fits of fainting soon led to the fatal event. It would have been desirable to know what was the state of the several internal organs after death in these cases; as this would have obviated at least one source of fallacy. We would not insinuate that the loss of blood was wholly unconnected with the fatal termination; but when it is remembered how many circumstances may concur, to induce the fatal termination in diseases for which copious blood-letting is requisite, it may safely become a question of some consequence, whether this evacuation was so much to be blamed. In another view of the case, the evil effects that result from carrying a powerful remedy to an extreme length, will be no argument against its moderate and judicious employment. Under all circumstances, though the effects of extreme discharges of blood cannot be denied to have a tendency in the mean time to impair the general strength, yet the physiologist has still to regret the want of a series of accurate and precise experiments on brute animals, and facts illustrated by dissection in the human subject, in order to form any correct theory of their mode of operation, and to apply such a theory to explain the phenomena of any given case.

In that part of Dr Hall's Essay, in which he inquires into the influence of various circumstances on the effects of loss of blood, the reader will find some interesting observations, which perhaps, however, savour a little too much of hypothesis. At least there is the same tendency, which we have already remarked, to draw general conclusions without a suitable collection of well established facts. It may indeed be answered to this objection, that the subject does not well admit the application of very precise measurement; for when an author reasons about the comparative susceptibility of infancy and of old age, and of strong and of feeble constitutions, it is not very easy to express in accurate or well defined terms, the relation which they mutually bear to each other. This objection, however, will not apply with equal justice to the nice and delicate distinction which the author draws between the comparative, or rather opposite susceptibility to the phenomena of reaction furnished respectively by inflammation, and the condition formerly described under the name of intestinal irritation. The latter he represents as particularly apt to lead to the phenomena of re-action;—to the former, he ascribes the power of protecting the system against this unmanageable state of disorder; in a state of intestinal irritation, throbbing is sooner induced unless its accession be prevented by a state approaching to syncope;—during the existence of an inflammatory disease, on the contrary, blood-letting is followed by little reaction, until the local inflammation is subdued, and the system is freely exposed to the uncontrolled influence of the loss of blood; and he contends that the phenomena of reaction assimilate readily with the constitutional effects of intestinal irritation, but are to a certain degree incompatible with those of inflammation.

All this may be very well, so long as we keep a general and distant view only of the supposed consequences of blood-letting, carried to a great extent, for the removal of visceral disease. But when we come to take a close and narrow inspection of the state of matters, we find that the facts supposed to afford the conclusions are few and imperfect, and that it is by no means easy to reconcile the conclusions with the facts which are well established and generally known. In the first place, we can assert, from personal experience, that we have seen all the phenomena of what is here termed morbid reaction take place after the prominent and perhaps the superficial signs of local inflammation had disappeared, but where longer observation showed that a chronic inflammatory action was still left; and if our single testimony be not corroborated by that of many of our readers, we request them to peruse some of the cases of Broussais and of Laennec. In the second place, we can also assert, from personal observation and experience, that we have seen all the phenomena of morbid re-action, in cases where it could not be traced to disorder or irritation of the intestinal canal; and where even Dr Marshall Hall would not say that the purging had been inefficient. Thirdly, and lastly, we have had more frequent occasion to ascribe these phenomena to the blood-letting being practised either too late in the disease, or in too small quantity;—to too early a return to nutritious food;—or to too early and imprudent exposure to the remote causes which had originally induced the disease;—than either to the misapplication of venesection, or to the great extent to which it was carried. The proof that this was correct, was found in the alleviation or disappearance of these symptoms of re-action, on withdrawing the unfavourable change of food, and preventing the operation of the injurious agents.

We will not conclude our remarks on this second Essay without expressing the hope that Dr Marshal Hall will not fail to redeem the pledge given in the close of it, to investigate the *organic effects*, and especially the *remedies* of loss of blood, by a series of experiments.

In the third and last Essay on Exhaustion and Sinking from various Causes, Dr Marshall Hall treats of this subject, in relation, first, to infancy or early life; secondly, to old age; thirdly, to several diseases; and, fourthly, to certain causes of exhaustion.

On the first of these divisions of the subject, the matter is simple enough. Every one knows that even of good things we may have too much, and of none more than medicine. It must be superfluous surely to say, that even useful remedies may be carried to a greater extent than is either expedient or requisite; and that it is not absolutely impossible to die of the doctor. Such a consummation, though most devoutly to be deprecated, there is reason to fear may happen, and perhaps does happen more frequently with infants than with any other of the categorical individuals of the human race. It is at this time of life that many of the actions of the living body are most excitable, most readily deranged, or most easily adjusted. A very slight and trivial cause may beget most potent effects; and the resources of the system are so great, and so various, and so inexhaustible, that some of the worst and most serious of these effects may be counteracted by means, the existence and power of which were equally unknown and unthought of.

Impressed with these circumstances, we are not surprised at the picture which the author before us draws of the state of exhaustion in early infancy, of the state of extreme weakness and irritability consequent on bleeding for croup, or on the unexpected effects of a violent cathartic; nor at the beneficial influence resulting from the exhibition of brandy. But as even the case in which this stimulant was administered, did not recover, and as no light is thrown on the original disease, (bowel complaint), or on the alleged injurious effects of the treatment, by dissection, we are quite as much in the dark as ever about the actual cause of death. The case of scarlet fever, we must be excused from saying, is most improperly placed in this section; for it is manifest that the infant died of the disease and its consequences, not of any treatment pursued, or any remedies administered.

With the subject of exhaustion, or sinking in old age, our readers are already familiar, by means of the Essay of Sir Henry Halford, or the Climacteric disease, in the fourth volume of the Transactions of the College of Physicians; * and from this Dr M. Hall has adopted part of the description. Of this Climacteric disease we know not well what to say; for if we say what we think, we may, by possibility, offend the learned President of the Royal College, and, through him, the College at large; and if we shape our opinions to our dutifulness, then are we no true men, but time-servers, and worshippers of the fashion of the hour, and offering a sacrifice of truth on the altar of authority. We shall simply ask a question :—Does this Climacteric disease never depend on affection of some internal organ? Does it consist of symptoms merely without a cause? At a more convenient season we may speak of this matter.

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* Transactions, vol. iv. pp. 318-320.

On the subject of sinking in certain diseases, Dr Marshall Hall has done little more than give the interesting observations of John Hunter on dissolution;* and as this may be perused in the work of that author itself, we hold it no part of our duty to stay further question. We cannot quit the subject, however, without remarking, that we see no ground for the refined distinction which Dr Hall attempts to introduce in talking about the sinking state, or the final and fatal termination as it appears in typhous fever and enteritis, dysentery, cholera, and pneumonia. All diseases, if not opposed by seasonable and powerful checks, natural or artificial, are liable to terminate in death; and though, physiologically speaking, it is a matter of curiosity to know in what manner death takes place in each, yet we do not see how this point is to be settled, by ascribing it, in general and sweeping terms, to the process of sinking, as it has been named.

Upon the whole, these brief Essays contain much curious matter of reflection and inquiry, and should be perused by all professional readers. We must say, however, that they are greatly too hypothetical; and what is more dangerous still, their hypothetical parts have, in our opinion, not a very good tendency. They may give rise to fears about the use of the lancet and the depleting system in general, which, we doubt not, will produce more harm than all the good likely to result from them. This, however, seems to be the fate of medicine as a theory, and as an art. From one extreme we vacillate to another; this year blood-letting to a prodigious and unheard of extent is practised in all diseases indiscriminately; the next it is denounced as a perilous and pernicious mode of practice. It is not enough to quote the old advice, *medio tutissimus ibis.* It is requisite to remember, that no remedy will prove a remedy, unless when used seasonably and judiciously. The physician who would find the truth, must avoid slavish imitation, and keep his mind unbiassed by the sophistry of partial conclusions from equivocal facts, and unseduced by the impetuous torrent of daily fashion.

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* Hunter on Inflammation, pp. 804, 506. 507.

XI.

The Anatomy of the Fœtal Brain; with a comparative Exposition of its Structure in Animals. By FREDERIC TIEDEMANN, Professor in the University of Heidelberg, Member of the Academy of Sciences of Munich and Berlin, &c. &c. *Translated from the French of* A. J. L. JOURDAN, by WILLIAM BENNET, M. D. *To which are added, some late Observations on the Influence of the Sanguineous System over the Development of the Nervous System in general. Illustrated by Fourteen Engravings.* Edinburgh, printed for John Carfrae and Son; and Longman, Hurst, Rees, Orme, Brown and Green, London, 1826.

WE are happy to see an English translation of the work of Professor Tiedemann. The subject of his work, though interesting to the physiologist and anatomist, is abstruse; and the circumstance of the original being composed in a language not very generally understood, and still less generally read, must undoubtedly operate in rendering the knowledge of the researches of Professor Tiedemann by no means general or familiar. The French translation of Mr Jourdan was certainly calculated to diminish the weight of this objection; but still it was in a foreign language,—a circumstance which never fails to operate powerfully in discouraging from the study of a scientific inquiry.

The present work, therefore, has the merit of depriving the English reader even of this excuse for his indifference; and we trust that it will not fail to attract the attention which it deserves, and to make the interesting and important researches of the Professor of Heidelberg generally, if not minutely known among our countrymen. That it ought to do so, we infer from the manner in which Dr Bennet has performed his duty of interpreter. The translation is written in a clear, easy, and unaffected style, and shows that the translator is well acquainted with the subject. It is, however, to be regretted that it was not done from the original German; for it is hardly in the nature of things, that the translation of a translation can be so correct or so faithful as the translation of an original.

Dr Bennet has added several explanatory notes, derived chiefly from the facts disclosed in the recent experiments and dissections of M. Serres. He has also added at the end of the volume, from the work of the same diligent and laborious inquirer, some good observations on the influence of the sanguiferous system in contributing to the development of the brain and its parts. As a specimen of the style and manner of the translation, and also as containing some curious information, we subjoin the following short extract.

"We perceive three distinct centres of formation, three different sources of development of the central part of the nervous system. If we adopt the idea of Willis, we suppose that the nervous system proceeds successively from the periphery of the cerebral hemispheres and cerebellum towards the spinal marrow. If that of Malpighi and Gall, we must imagine that the brain radiates from the spinal marrow, as the branches of a tree from the parent trunk. But these opinions are not exact. The principal parts of the brain take very different directions in their progress of development; the cerebellum is formed from behind forwards, and the cerebral hemispheres, on the contrary, from before backwards.

"When we examine the arteries which embrace the brain and give rise to its production, we can perceive the cause of this peculiarity of direction; for we observe that the vertebral artery enters at the posterior part, and is directed from behind forwards, according to the manner of formation of the cerebellum. If, on the contrary, we follow the direction of the internal carotid, we observe that, after its having formed those double turns along the cavernous sinus, it gains the anterior part of the brain, and proceeds from before backwards, agreeable to the mode of development of the cerebral hemispheres.

"Thus the development of the cerebellum follows the direction of the vertebral artery, and that of the brain the course of the internal carotid. Hence it follows that the corpus callosum is developed from before backwards, according to the gradual progress of the artery of this part. Hence, also, the anterior part of the fornix follows the same direction, while the posterior part, which is developed under the influence of the posterior cerebral artery coming from the vertebral, proceeds in its formation from behind forwards.

"From this union of the vertebral and carotid arteries, results the general relation observed in the vertebral animals, between the development of the cerebellum and that of the cerebral hemispheres." pp. 290–291.

The rest of this short but interesting tract is occupied with illustrations of the same general principle; and besides explaining the relative progress of the different parts of the nervous system, tends to establish what had been only vaguely and imperfectly understood, that the arteries are the great agents of nutrition and growth in the brain and nerves, as well as for other parts of the living body.

The engravings by Lizars are tolerably successful copies of those published by the author. The mechanical execution of the book is also so excellent as to merit especial notice. It is one of the best specimens of typographical art that we have yet witnessed among medical publications.

PART III.

MEDICAL INTELLIGENCE.

An Annual Nosological Table of the Diseases, which have occurred at Bolton-le-Moors, between the 1*st November* 1824, *and the* 31*st October* 1825. By J. Black, M. D.

Genera Morborum.	November.	December.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	Total of each Genus.
Febris Tertiana									1				1
—— Synochus	4	1	3	3	1	1	1	8	4	8	10	3	47
—— Typhus	1	1	2					4	2	4	5	2	21
—— Infantm. Remittens.	5	4	1		1	1				6	2		20
Rheumatismus	6	6	5	6	10	9	17	16	11	10	8	4	108
Podagra		1	1			1						1	4
Variola								1					1
Rubeola	1	1											2
Mania								1				1	2
Apoplexia	1	1						1		1		1	5
Paralysis	1	3			1			2	2	4	1	2	16
Epilepsia & Convulsio	3	1		4	4	1	2	1	4	1	6	6	33
Cephalea		4	5	3	2	9	1	6	2	7	2	7	48
Cephalitis			2		1								3
Hydrocephalus		1				1	1						3
Ophthalmia, & Eye Complaints	8	11	5	8	6	8	8	16	18	12	10	13	125
Cataract					1		1	1					3
Otitis										1		1	2
Cynanche				1	4		3		5			1	14
Bronchocele	1	1		1	1				3	1	2	1	11
Catarrhus	9	12	7	10	8	7	9	4	3		7	8	84
Pertussis			1	7	6	5	6	8	7				40
Pneumonia & Bronchitis	5	6	3	1	7	3	2	7	4	7	5	7	57
Asthma	2	2			7	5		1	4			1	22
Hæmoptoe					4					1	1		6
Phthisis	4	3	12	7	6	13	9	12	1	8	4	8	87
Cardiaca	3	2			3	4	1	7	2	2	2		24
Hydrothorax				2									2
Hydrops, (Ascit. et ovar.)	1	2	2	1	1	2	1	4			2		16
Anasarca		1			1	1					1		4
Hydrops Uteri			1	1									2
Cœliaca	32	27	30	26	34	37	33	36	44	24	33	22	378
Enteritica	3			7	8	5	11		7	10		11	60
Enteritis	1			1	4			3				1	10
Hepatitis	2		1	2	1	1	2		1		1		11
Cholera									1	4	8	8	21
Diarrhœa	8	6	4	5	7	10	3	10	16	24	52	21	146
Dysenteria		4											4

Genera Morborum.	November.	December.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	Total of each Genus.
Icterus				1	3	2		1	1		1	1	10
Nephralgia		1						1			1		3
Tabes Mesenterica			1								1		2
Hæmatemesis		1					1						2
Epistaxis		1											1
Colica		1	2					1		2			6
Hernia Incarcer.								1			1		2
Menorrhagia	3	4		2	3		6		3		1		22
Amenorrhœa			1	2	2	1	2		2		4	1	15
Dysmenorrhœa			1		3	3	6	1		2		1	17
Hysteria	2		2					1	1			1	7
Hysteralgia									3	1			4
Chorea							1	2		1			4
Scirrhus Uteri									1	1			2
Carcinoma Uteri											2		2
Scirrhus Mammæ												1	1
Cancer oris et labii			1						1				2
Hæmorrhois	1	2									1		4
Curvature, lateral, of Spine							1						1
Ischuria			2	1		3		1				1	8
Leucorrhœa			1	1									2
Diabetes											1		1
Gonorrhœa		1	2		1		2	1	1		1		9
Syphilis	1		2	1				1		1	1	1	8
Scrophula Externa	6	4	2	8	5	2	3					1	31
Marasmus	1			3	1	1	4	7	3	4	1	5	30
Erysipelas		2					2	1	2	1	1		9
Urticaria										1			1
Herpes	3	7	4	3	4	6	6		2	4		1	40
Lepra Vulgaris	3	1									2	1	7
Crusta Lactea	2					1	1						4
Scabies			2		4				1				7
Strophulus											1		1
Purpura			1	1				2					4
Intertrigo				1			1						2
Porrigo						3					3		6
Spasmus			2				2						4
Neuralgia			1			2							3
Morbus Coxarius				1									1
Abscessus Lumbalis					1								1
Suppur. Mammæ						1							1
Ruptured Urethra							1						1
Accident	2		1			2			1				6
Slight and Anomalous Complaints	20	6	18	11	16	17	6	17	13	13	13	15	165
Total of each month	145	135	131	132	172	166	156	187	177	164	178	160	
Grand Total for the Year													1901

Remarks.—This Table, which embraces the above cases, as they indiscriminately occurred among all classes of the inhabitants, may be held as a general map of the relative proportions and characters of the annual list of diseases; and, on comparing the registers of the several months with each other, it is somewhat interesting to remark the rise and fall of the nosometrical scale.

As the town and neighbourhood, during the above period, were not afflicted with any epidemic disease, with the exception perhaps of an unusual average of cases of cholera and diarrhœa, the Table also presents a tolerable picture of what may be called the indigenous character of the complaints and diseases of the place. The year before also exhibited a more marked exception to the general nosography, in the very numerous and fatal majority of cases of rubeola. Indeed I have never witnessed, nor have I read of a more inveterate state of the disease. Symptoms, sometimes mild at first, soon put on a most serious character; and, on the decline or retrocession of the eruption, fatal effusions in the brain and chest, were nearly as uncontrollable as they were frequent. The mortality from this disease among children was increased by nearly seventy a month during the spring of that year.

The synochal fevers in the present Table present little difference from the general characters of this genus, and were all more marked for derangement of the cœliac organs, than for any determination to, or affection of, the brain. It appeared, that of the greater part of those which occurred during the autumnal months, slight *choleric* or diarrhœal attacks had been the forerunners, the convalescence from which was considerably retarded by the supervention of fever. These latter varieties of fever would not bear great evacuations, but an attention to the supposed irritation and capillary debility of the mucous membrane of the intestines, at length led to the restoration of health in all of them.

The typhous cases were much more serious and fatal. From not seeing many of them in the first stages, the disease had generally, on visiting them, entrenched itself in some organ or viscus, and of these the abdominal viscera seemed most involved. With three exceptional cases of well marked cephalitic typhus, they might all have been arranged under the abdominal species,—the lungs seldom showing any prominent feature of concomitant or precursory disease. The report of other practitioners has also given nearly the same account of other cases of fever, which have occurred during the year in the place, though the disease has never prevailed epidemically in the town or immediate vicinity during the last year, but has confined itself to a few families among the lower classes of working people, and to a few scattered cases arising from individual importation, or privation of the necessary food and stimuli of life.

The genus Rheumatismus is chiefly filled with cases of the chronic species; and even the first impression of last summer's heat seemed to increase this class of complaints, from the excitement being apparently too great and too sudden for the debilitated tissues of the joints and ligaments;—but as the weather became more steady in its heat, and the body more accustomed to a high temperature, these complaints diminished in the scale, so that if the warmth had continued, they would no doubt have become as unfrequent as in a tropical climate. A few of the more acute cases were treated successfully with calomel and opium, given in large doses, until the mouth was tainted, when a most copious and seemingly a critical emulgence of the liver took place, (with the assistance of ol. ricini), even when no hepatic *remora* or congestion was believed to exist. However this actually might have been, the fever, pains, and immobility forthwith disappeared. Other cases were again brought to a desired issue by colchicum, an occasional small bleeding, and sudorifics. How far abstinence from much general depletion, in acute rheumatism under other circumstances, is to be followed, is a matter of separate consideration, as I have myself drawn much blood in this disease, in cases occurring in a pastoral country, and in the fleet; but I am inclined to think I would not insist so much on it as I have done. Some of the cases of Paralysis exhibited a good deal of interest, as the disease affected the different nerves about the face, and those of the extremities; and the remark, that these cases were very much increased during the hot weather of autumn, is corroborated by the observation of other practitioners in the neighbourhood.

The heat of the season also seemed to have increased and exasperated the cases of ophthalmia, but many of the acute cases, included above, were of the purulent species affecting infants; which cases, on first application, had a most alarming and inveterate appearance; but, except when the application was negligently late, to the generation of corneal ulceration, recovery was generally rapid. In all of this species nearly, a venereal contamination from the mother could be discovered; or, at least, she or her friends owned to a discharge of the whites when confined. Psorophthalmia is very common among all ages and sexes of the lower classes, and many a pound of *golden ointment* is consumed by them during the year. The hooping-cough has been very prevalent this last summer, and it sometimes led to fatal effusions and infiltrations of the lungs of infants. The Table shows its relative appearance and intensity, and I have besides not heard of any cases of consequence in the place since. The genus *Cardiaca* of the Table embraces the symptomatic, functional, and organic diseases of the heart; and the frequency of these complaints, even among the manufacturing classes, seem to claim increased attention and interest on the part of the moralist, as well as of the physician.

In the list, by *Cœliaca* is understood all that multitudinous and proteous class of complaints, commonly called dyspeptic and bilious; but the term is meant more definitely to comprehend all the derangements and diseases of the organs and viscera supplied by the cœliac artery, which are unaccompanied with marked fever, or any exclusively prominent and topical disease; and not in the more generally nosological sense of the cœliaca of Dr Good. From the peculiar habits and modes of living among the operative classes, in manufacturing towns and districts, these complaints of the cœliac organs are very frequent, and seem to be a good deal owing to the repeated and alternating excitement and atony of the digestive organs, arising from the use of irregular diet and drink, confined employment, and an injurious physical education of childhood and youth. A great majority of these complaints assume many morbid masks and characters, and may be represented, in this simulating point of view, under the appropriate term of the *Mimoses* of Dr Hall.

Though the larger blood-vessels and the portal circle seemed very often to labour under an impaired tension and sluggish circulation, giving rise to cramps, dull pain, or what is termed *doneness* by many, costiveness, ischuria, and fulness of the epigastrium, yet the mucous membrane of the stomach and intestines also seemed, in many cases, in a state of turgescence and irritation, producing acidities, cardialgia, flatulence, vomiting, and an irregular and vitiated state of the secretions and excretions of the bowels.

The next genus, *Enteritica*, is intended to comprehend those uneasy, painful, congested and constricted affections of the bowels, where there is a little irritation of the system, but no symptomatic fever. An aggravation, however, of the pathological character of this genus will make an easy transition to the next, Enteritis. All these classes of abdominal complaints are presumed to be much engendered and aggravated, among all sexes and ages almost of the middle and lower classes, by the habitual potation of malt liquors; which species of daily drink by people not exposed to operations in the open air, and who are engaged in the more sedentary employments, seems to produce an atonic plethora of the blood, and a diminished tension in all the blood-vessels of the great cavities. Hence, frequent and sudden congestions and inflammations, speedily running to effusions and fatal *engorgements* of the viscera. In other parts of the kingdom, where ale is not drank as an article of diet, and but seldom, by the same class of people, but where ardent spirits are used as the occasional beverage, inflammations, from my observation, have put on a more tenacious character; and successful bleeding could not only be carried to a much farther extent, but could be had recourse to at later stages of the complaints—the resiliency of the blood-vessels being apparently more perfect.

Of the genera Cholera and Diarrhœa, it is seen when their increments took place, and how, it seems, the cases of diarrhœa rose in the nosometrical scale, at the expense of a few of the cœliaca; of whom it was natural to suppose they would experience, more than other morbid predispositions, the epidemical influence of the season, and so have for a time their nosological character more or less changed.

Without enlarging any further this commentary, or taking any notice at present of several very interesting cases that numerically stand in the Table, I shall merely advert to the disappearance of the cases of scrofula externa (affections of the conglobate glands) as the season rose in temperature.

In conclusion, it may be expressed, how desirable it would be to compare a number of similar tables from different towns and parts of the kingdom, in order that hasty conclusions may be corrected, and legitimate ones formed; and that a topographical chart of disease may be constituted on an extended scale, either for separate seasons, or for the general nosography of the year.

Bradshawgate, Bolton-le-Moors,
12th Nov. 1825.

Prize Question.—The Royal Medical Society of Edinburgh propose, as the subject of their Prize Essay, the following questions:—

1st, What is the respective agency of the veins and the lymphatics in the process of absorption?

2d, By what means or mechanism do these vessels accomplish this process? What are the processes which show that the substances absorbed are taken up by open mouths or orifices, or pass through the coats in the manner of imbibition or transudation?

3d, Is there any reason to believe that the individual animal tissues possess a distinct power of absorption, or that this process is influenced by the nature of the animal tissues?

The sum of Twenty Guineas, a Medal, or a set of Books of that value, will be given to the author of the best Dissertation on the subject proposed by the Society, for which all men of science are invited to compete.

The Dissertations may be written in English, French or Latin, and must be transmitted to the Secretary on or before the 1st of December 1826; and the adjudication of the Prize will take place in the last week of the month of February following.

To each Dissertation must be prefixed a Motto, which must likewise be written on the outside of a sealed Packet, containing the name and address of the author. No Dissertation will be received with the author's name affixed; and all Dissertations, except the successful one, will be returned, if desired, with the sealed packet unopened. NICOLSON BAIN, *Secretary.*

Conclusion of Dr Oppenheim's Observations on the Present State of Medicine in Italy.

The Papal Dominion.—There are two medical schools in the dominions of the Pope, viz. one at Bologna, and one at Rome.

Rome.—The University at Rome is named "*Della Sapienza,*" and has fourteen medical professorships. This university has no museum whatsoever. The medical clinic is in the "*Ospedale di St Spirito,*" where practical anatomy is also taught. The surgical clinic is in the hospital at "*St Giacomo in Angustia.*" There is no institution for the instruction of accoucheurs. The internal arrangement of the Roman hospitals is so peculiar, that it deserves particular notice. They are altogether ecclesiastical institutions, formed according to the notions of churchmen, and destined to serve rather as asylums for the administration of spiritual consolation, than for the cure of diseases. Accordingly, the physicians and surgeons are persons but of secondary importance in a Roman hospital, while the priests and confessors enjoy the chief authority! They alone are the resident officers; to them the admission of a new patient is first communicated; and they administer the first remedies, confession and the sacrament. Chance must decide upon the remaining part of the cure; for after having taken care of the soul, they concern not themselves about the cure of the body! The hospitals are small, but, on the whole, rather clean. The bedsteads are generally made of iron, some with, and some without curtains. Some of these hospitals are situated in the most unhealthy parts of the city. They are nine in number, and altogether contain about 2000 beds. In some there are separate wards for consumptive patients, for the opinion that consumption is contagious, is universal in Italy. In the lunatic asylum are 400 patients, on whom the whip and the chain are not spared! Some of the hospitals cannot be visited by strangers, except permission has been granted by the Pope, a favour of which his Holiness seems to be very sparing. All the convalescents from the different hospitals are brought to that of the Holy Trinity, for the purpose of enjoying the benefit of a nutritious diet. Dr Oppenheim finds fault with this arrangement; and we agree with him in thinking, that the convalescents might be well fed in the different hospitals, without going to the trouble of removing them to this general convalescent hospital, where, after all, they are only permitted to remain three days!

The diseases of every nation are necessarily much influenced by the customs and domestic habits of the people, and the nature of the climate. He who has not been an eyewitness of it, cannot form any idea of the uncleanliness prevalent in the South of Italy. The stranger, on his first arrival at Rome, is amazed at seeing whole groups of people, "*gruppi dei otiosi*," consisting of fathers, mothers, children, and friends of the family, all employed in performing for each other, an office which we shall not name. Suffice it to say, they use their fingers for purposes elsewhere performed with combs! This custom is so general, that it has, as a matter of course, occupied the pencil of the artist: and in the magnificent collection of pictures at Florence, is one in which Venus is seen thus elegantly employed on the head of Cupid! So much are the inhabitants accustomed to sleep two in one bed, that when two strangers arrive at a country inn, and require two beds, their demand is considered unreasonable. The peasants of Rome and Naples look upon washing and cleansing the skin as quite unnecessary; and the upper ranks are not less negligent of the bath, than the ancient Romans were attached to its use. The great number of church holidays serves, indeed, in a certain degree, to keep the city from utter filth, for the streets through which the religious processions are to pass, must be previously swept.

In so warm a climate, this utter neglect of cleanliness necessarily produces an abundance of cutaneous complaints; and accordingly, the hospital "Della St Maria," containing 400 beds, is insufficient for the accommodation of patients so afflicted. *Tinea capitis* is treated in this hospital in the following singular manner. The head is first smeared with butter, for the purpose of softening the scabs. When the scabs are removed, the head is shaved, and all the roots of the hair pulled out with a broad tweezers. The next step is to make forty or fifty incisions in the scalp with a razor. The free flow of blood from these incisions is favoured by making the child sneeze. The head is finally washed with cold water, and then rubbed with rancid oil. This *cutting* and *plucking* is repeated every four or five days, as fast as the hair begins to appear. This *cure*, which they commend for being *simple* and *radical*, lasts generally for six or eight months, and in obstinate cases for one or two years!

Vaccination is again much neglected under the present Pope, and of course small-pox is on the increase. There were about 70 small-pox patients in the hospital at the time of Dr Openheim's visit. We shall not enter into the sources of the "*aria cattiva*," which renders Rome so unhealthy, but merely remark, for the benefit of such of our readers as may intend to visit that city, that its influence is most severely felt during the months of June, July, August and September. The miasma produces, besides common agues, a very fatal species of fever, which is termed the malignant ague. "The patient becomes at once weary and weak, complaining of heaviness of his limbs, heat of skin, dull headach, and confusion of ideas, &c. The looks are wild, the face oftener pale than red; and, even when it is flushed, a yellowish white tinge is perceptible near the angles of the mouth. The belly is often tender to the touch, and the right hypochondrium swollen. The patient is sometimes costive, but not unfrequently diarrhœa is present from the beginning. Enough has been related to place it beyond doubt, that this fever, at its commencement, is of a gastric character, and is attended with an inflammatory affection of the liver. After the above symptoms have continued for some time, the fit commences with a violent rigor, which is followed in an inconceivably short space of time with a general and excessive disturbance of the whole nervous system, picking the bed clothes; *subsultus tendinum*, the most violent delirium, and a low muttering sort of raving, succeed each other rapidly, and without any apparent regularity. In short, the disease at one moment wears the aspect of a fever attended with excitement, and at the next has all the characters of typhus in its latter stages; and these two forms alternating with each other, seem, if it were possible, combined in the same patient. After the fit the patient feels a greater degree of depression than before. Vomiting often comes on at the height, or towards the end of the paroxysm. The second fit commences five or six hours after the first; and the third begins after an intermission of about the same duration, unless indeed, which not unfrequently happens, death has already closed the scene. *The third fit is always* *fatal!* Peruvian bark exhibited in the largest possible doses, is the " *sacra ancora*' on which the Roman physicians place all their hopes. During the fit blisters and sinapisms are applied to the extremities, while the head is assiduously cooled by means of cloths dipped in cold water. The instant the first fit has ceased, bark is given, and that with the greatest possible diligence, as they do not know the moment a second fit may commence. It is always a favourable symptom that the " patient bears the bark well, but it too often happens that the stomach immediately rejects it. When this is not the case, four or even six ounces of bark are exhibited in the course of the day. The second fit is then so much diminished in violence, that its accession causes but little disturbance, and the patient is saved. The progress of this disease is most rapid in strong, robust, and plethoric habits. Relapses also frequently occur. In 1825 the proportion of recoveries to deaths was as 85 to 15; in other years it has been as 80 to 20. All other remedies have proved ineffectual in this fatal disorder. Venesection, emetics, opium, &c. seemed only to hasten its fatal termination, so that the physicians now place no dependence upon any thing except bark, which is used in immense quantities at Rome. The quantity used in the Ospedale St Spirito often amounts daily to 40 or 50 lbs."

The latter statement of our author, we cannot help observing, accords ill with his previous sweeping censure, concerning the inattention prevalent in the Roman hospitals, with regard to *the bodily complaints of the patients*.

Some successful trials had been made at Rome with the sulphate of quinine, both in this fatal, and in the common forms of ague. When there are symptoms of a deranged state of the alimentary canal, the Romans place no reliance on emetics, but cleanse the *primæ viæ* with one drop of croton oil, previously to exhibiting the bark.

The following rules are laid down by the Roman physicians, for strangers who remain at Rome during the sickly season:—They must get up at six o'clock, and, having made a light breakfast, on biscuits, coffee, &c. they may go about their business until after eleven. At one o'clock they dine, and ought to sleep for a few hours after dinner. It is reckoned dangerous to be out at noon, sunrise, or sunset. Two hours after sunset, a walk is recommended, after which a light supper may be taken. At twelve they ought to go to bed, and should sleep with but little bed-clothes. The windows of the bed-room should be kept shut during the night. Strangers are likewise recommended to indulge in cool drinks, and to be very abstemious with regard to wine, which ought to be diluted with water.

" *Phthisis* is not common at Rome, and its introduction has been attributed to the English! for it is universally believed to be contagious. When a consumptive patient dies, his clothes, furniture, and bed are always burned. There exists too a Papal Bull prohibiting the sale of such articles. Scrofula is not uncommon at Rome, and calculous complaints are of rather frequent occurrence. Professor Sisco, who has performed the operation of lithotomy on more than fifty patients, follows the method recommended by Cheselden, i. e. the lateral operation; his success has been considerable, and he objects in strong language to the recto-vesical operation of Vacca. The Roman surgeons boast of great success in strangulated hernia. *Sisco* never tries to heal the wound by the first intention, because he considers the cure by suppuration as the only radical one, as it produces a complete solidification of the parts and thus prevents the necessity of afterwards wearing a truss.

Aneurisms are not common in Rome; their cure is generally attempted, according to Vansalva's method. The Roman surgeons *have hitherto never ventured to tie the artery in this disease, but always proceed at once to amputation when Vansalva's method fails!* and yet Rome is scarcely three days journey from the residence of Scarpa!

In Syphilis mercury is now used both externally and internally. *Buboes* are never permitted to burst spontaneously; they are always opened with the lancet.

The doctrine of contra-stimulus has fewer advocates in Rome than in any other Italian city.

Bologna has 60,000 inhabitants, an university, two civil hospitals, an orphan house and a work-house. *Spedali della Vita* contains about 500 beds. The wards are spacious, well ventilated and clean. Professors Comelli and Tommasini superintend the medical clinic, and Professor Venturoli the surgical. It is unnecessary for me to detail the medical practice in vogue here, as it is generally known, and the doctrines of contra-stimulus are already sufficiently familiar to the medical world, through the medium of a journal published in Bologna, under the title, "*Giornali della nuova Dottrina Medica Italiana.*" Without discussing the merits or demerits of this doctrine, I may however remark, that the very large doses of medicines which its advocates are in the habit of exhibiting, have universally the effect of rendering the system so difficult to be acted on, that the doses must be constantly increased, in order to produce any effects. Thus I saw a man under treatment for abdominal disease, on whose bowels half a drachm of jalap, and four ounces of castor oil, had not the least effect, until their action was assisted by a purgative enema! We pass by the other hospitals, as affording nothing worthy of remark, except that their internal economy seems better regulated, than that of other Italian hospitals.

The university reckons about 600 students. It is large and beautifully built, and contains not only a good museum of natural history, but a tolerable anatomical collection. The preparations with which it has been enriched by the present Professor of Anatomy, Mandini, are very fine, as are also those made by Professor Quadri, who formerly taught here, but now resides at Naples. In the wax works I was much struck by the beauty of the pieces representing the muscles; they are the works of Lelli and Madame Penarolini. The library contains about 150,000 volumes. The librarian, Professor Mezzofanti is distinguished in the literary world by his uncommon talent for languages. The Botanical Garden is the best I saw in Italy. *

The Kingdom of Naples.—A glance at the state of general literature in Naples, will best explain the state of medical science in that city. All foreign books, even those which have received the sanction of the Roman censors, are subjected to the revision of the Neapolitan censors. If after a scrupulous examination nothing is detected unfavourable to the King or Church, the publication of the book is permitted, and a tax of four carolines on each volume must be paid by the publisher. This sum is exorbitant, when we consider the cheapness of Italian books. So heavy a tax is likewise imposed on public prints, that it amounts to a prohibition of all foreign journals. These regulations render the advancement of science so slow in Naples, that what has long since become obsolete in other countries, is there considered as a literary novelty. The Neapolitan physicians find it therefore impossible to keep up with the modern improvements in medicine, and consequently their practice is formed on antiquated models. Luckily for the inhabitants, the climate is so mild, that they seldom stand in need of medical aid, and may in general leave their complaints to the cure of the *vis medicatrix Naturæ.* Little can be expected from an university in such a country, even though it boasts of such men as Vulpes, Quadri, and Lanza. The hospital accommodation too at Naples is quite disproportioned to the number of the inhabitants, which amounts to 450,000.

Dr Oppenheim then proceeds at some length to describe the Lunatic Asylum at Aversa, a little town, situated eight miles from Naples, on the road to Capua. This institution excels, indeed, all others in Italy, but still falls far short of similar institutions in England and Germany. The only peculiarity we think worth relating, arises from the habits and genius of the Neapolitan people. In English mad houses, much reliance is placed in affording means of employing or amusing the convalescents. With this view, we provide the lower classes with the means of following their ordinary trades and occupations, while we offer to the rich the recreation of cultivating a garden, or reading in a library. At Aversa, what are the objects of amusement? Billiard tables, music, various petty national games, puppet-shows of all descriptions, and a variety of toys!!

We would almost believe its inmates to be boys, not men. But men it seems they are, at least in Naples, where even the upper classes spend the whole day in the coffee-houses or at the theatre, while the lower orders visit the puppet-show at seven o'clock in the morning, after which they dance the "*Tarantala,*" and play the *Morra.* This systematic trifling and habitual idleness afford a sufficient excuse for the indignation of the Priest described in Kelly's Memoirs, who, having dispersed a crowd of Lazzaroni assembled around a puppet-show, held up the crucifix, with which he had dealt his blows, and cried, "Ecco il vero Polcinello,"—an exclamation, which at first appears ridiculous, if not impious, but really conveys a most degrading idea of the people whom he addressed.

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* Over the door of the Botanical Garden at Pisa, was formerly inscribed "*Hic Argus, sed non Briareus esto.*"

The *Lombard State* forms at present a province of Austria, and the constitution of the two universities it possesses, is modelled entirely according to the plan of that of Vienna. A certain course of study, which lasts for five years, is laid down, and no deviation from this plan is permitted. The disadvantages of a restrictive plan of study, which assimilates an university to a great school, are too obvious to require comment; and we cannot therefore help regretting with Dr O. that it should have been lately introduced into France. Our regret is increased by observing, that this plan does not include a branch of medical study, to which we owe our chief progress in anatomy and physiology, and to whose aid we are to look for important additions to pathology, we mean comparative anatomy. When, says Dr O. we find so important an omission, we must indulge in gloomy anticipations concerning the future progress of science in a State, which was formerly so distinguished in comparative anatomy, and produced such men as Spallanzani, Valsava, Lancisi, &c.

Padua, a town with 20,000 inhabitants, has one spacious hospital capable of holding 300 patients. The wards are clean and well ventilated, and the beds are placed at a sufficient distance from each other. The clinical wards contain 24 beds. The medical department is superintended by Professor Brera; the surgical by Professor Ruggieri. The university reckons about 700 students. The building is very beautiful, and originally a palace, planned by Palladio. The cabinet of natural history is tolerable. Fabricius ab Aquapendente, Proper Alpinus, Morgagni, and other celebrated men, once ornamented this school. Brera, the present Professor of the practice of physic, is a zealous advocate of the system of contra-stimulus, and of course propagates this doctrine among his pupils. It has, however, found fewer advocates among the private practitioners at Padua, than among those at Bologna.

Pavia has 22,000 inhabitants. The Town Hospital is well situated and very roomy, and contains 400 beds. It is better calculated for the purposes of an hospital than any other institution in Italy, having been originally built for the accommodation of the sick, and not, like the rest, for a monastery or a palace. There are four clinical wards, viz. one for medicine, one for surgery, one for diseases of the eye, and one, (as the Catalogue has it) "per la istruzione di maestri in chirurgia e flebotomia." Connected with the hospital, is a small institution for lying-in women.

The university is attended by about 400 students.

The building is large, and the architecture fine. No university in Italy can boast of such rich cabinets of natural philosophy, chemistry, natural history, and anatomy. The latter, commenced by Rezia, received many valuable additions from the celebrated Scarpa, lately a Professor at Pavia. Burserius, Tissot, and the two Franks, formerly taught at Pavia.

Tabular

Tabular View of the Medical Sciences taught at the chief Italian Universities, and of the Professors by whom they are Taught. *

	Genoa.	*Pisa.*	*Rome.*	*Bologna.*	*Padua.*	*Pavia.*
Anatomy	Mozzini	Catellachi	Lupi	Mandini	Caldani	Panizza
Physiology	Do.	D.	Bomba	Medici	Gallini	Rusconi
Therapia Generalis	Batto	Morelli	Cetti		Giaccomini	
——— Specialis	Do.	—	Valentini	Tommasini	Brera	Chiappa
Materia Medica	Mongiardini	Bianchi			Della Decima	
Medicina Forensis	Do.		Falconi		Fanzago	Salvadori
Surgery	Garibaldi	Vacca-Berlinghieri	Trasmondi	Venturali	Ruggieri	Borda
Clinical Medicine	G. Scassi	Morelli	Matheis	Tommasini	Brera	Hildenbrand
Clinical Surgery	Giudetti		Sisco	Venturoli	Dall' Oite	Cairoli
Botany	—	Savi	Donarelli	Bertoloni	Bonato	Rocca
Chemistry	—	Bianchi	Morecchini	Salvigni	Melandri	Manabelli
Natural History	—	Savi Junr.	—	Ranzani	Renieri	Mangili
——— Philosophy	—	Lichi	Barlocci	—	—	Configliachi
Institutes of Medicine	—	Barzelatti	—	—	—	
Do. of Surgery	—		—	—	Fabini	Cairioli
Pathology	—	Studiani	Fulchi	Ordonari	Della Decima	Chiappa
Midwifery	—		Asdrubali		Lamprecht	Bongiovanni

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* We have made some alterations on Dr Oppenheim's List, upon the authority of a medical friend, lately returned from a residence of some time in Italy.—Editor.

*Hereditary Tendency to Hæmorrhagy.**—Of three sisters, Theresa, Catherine and Cydonia, the last, Cydonia, had two children who both died young of measles; the second, Catherine, had ten children, of whom one son was subject to *Hemorrhæa petechialis*, and died at 25 of epistaxis, caused by a fall from a horse. Eight of the other children died under two years of age of various diseases. The eldest sister, Theresa, had three sons and five daughters; of these, two sons and one daughter died young, and one son and four daughters are still living. The son is perfectly healthy; one daughter has emigrated, the second had two children who died young, the third is unmarried and healthy, except liability to ecchymosis in her youth; and the last, Lucy, also healthy, but subject in her youth to ecchymosis and violent epistaxis, had three sons, all of whom have been cut off by haemorrhagy.

The eldest, John, when only fourteen days old, was carried off by spontaneous and uncontrollable hæmorrhagy from the navel, after it was perfectly cicatrized.

The second and third fell under the observation of Dr Elsaesser. At ten weeks after birth the second became affected with a tendency to ecchymosis, so that until his death, there were always present on various parts of his body, from five to twenty blotches of different sizes. At eight months old the first severe hæmorhage occurred from his gums, caused by a fall. After this he had frequent, obstinate, and excessive hæmorrhages from his nose, both from falls and spontaneously. When two years old, he almost bled to death from a slight cut on a finger. At three he had chin-cough, whose paroxysms were attended with copious expectoration, and frequent and violent epistaxis, but without vomiting. In all these hæmorrhagies the blood flowed quietly from all the surface, as from a sponge, thin in consistence, and difficultly coagulable; at first of a dark red, but afterwards of a pale colour, like the water in which flesh had been washed, and having a peculiar smell. The wounds commonly healed by the first intention, rarely by suppuration. When no hæmorrhage had occurred for some time, he became affected with evident signs of plethora, strong pulse, red tumid face, red ears, &c., but the occurrence of hæmorrhage did not seem to depend upon the season of the year, or age of the moon. It was singular that no hæmorrhagy took place from the punctures made in vaccinating him. When three years and eight weeks old, he wounded his right cheek very slightly on the 21st November 1822. A few drops of blood only escaped, and a crust formed on the wound. On the 5th December the crust came off after he had been using exercise, he bled alarmingly till midnight, and he sunk next day with his whole body of the colour of bleached wax. After death his right knee became remarkably red, and about twenty ecchymotic spots were found upon his body.

The third and youngest brother, born in 1821, seemed perfectly healthy, until he was eleven weeks old, when he began to cut his teeth. He was then affected with complaints of various kinds and striking muscular debility, like his deceased brother. Similar ecchymotic spots appeared, and under the same circumstances, but although he had chin-cough from May 1822, to February 1823, no hæmorrhagy took place, nor did he experience it from twice cutting his finger with a sharp knife. His mother on two occasions observed his whole abdomen to become blue, but the colour disappeared in about six or eight hours. He was vaccinated in 1822, without any irregularity in the progress of the pustule. On the 17th April 1823, he had his first hæmorrhagy in the form of bloody clots passed by stool. On account of his eyes and a bright red eruption like urticaria, he got on the 19th a purge of calomel, jalap and magnesia, which produced two florid bloody stools. On the 23d May he struck his head on a sharp corner, which produced a small but deep wound. At the time it discharged only a few drops of blood, and became covered with a crust. But on the 25th at 4 *a. m.* the wound, not larger than a pin's head, burst out and bled violently for nearly 30 hours, with intervals of an hour or two, until it was stopped with prepared sponge. It was remarked that the blood in this case also did not spring out, but oozed as from a sponge, at first dark red and viscid, afterwards florid and thin, and that as often as the child became warm in bed, the hæmorrhage was renewed. For some time before, his ears had become of a deep red colour, which frequently alternated with a waxy paleness. After the bleeding they became cold, and quite exsanguious. For half a year before, his skin also had been intolerably itchy. On the 30th of June at 7 *a. m.* he had two stools of black clotted and very fetid blood, and at 10 *a. m.* a third. He had no further motion till 1st July, when he passed by stool a large quantity of dark red clotted and very fetid blood at 1 *a. m.* and again at mid-day. In the evening his debility was extreme, and attended by convulsions. On the 2d a copious discharge of *florid*, somewhat lumpy, very fetid blood *per anum* brought on insensibility and convulsions, and he sunk about mid-day.

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* Observed by Dr Elsaesser near Stuttgard, Hufeland's Journal, February and September 1824.

The body was opened 28 hours after death. It was generally exsanguious like a wax figure, but here and there, especially on the thighs, separate spots of ecchymosis were visible upon the skin. The wound on the left side of the head was firmly cicatrized. Between the integuments and pericranium, there was a large suggillation on the crown of the head. The *dura mater* adhered firmly to the cranium; the cortical and medullary substance of the brain natural; no water in the lateral ventricles; the *plexus choroïdei* bleached and empty of blood, as were all the blood-vessels in the head; cerebellum also bloodless; some ounces of turbid liquid flowed from the spinal cavity. The viscera of the thorax and abdomen were almost bloodless, and very much blanched; the thymus remarkably so: from incisions made into the pale lungs, some quite thin blood escaped. Immediately above the trachea, in the angle between the *innominata* and carotid, was found some glands as hard as cartilage and of a yellowish white colour; the pericardium contained little serum; the structure of the heart was quite natural; a watery blood that tinged linen of a pale and red colour, as flesh-water does, flowed from the large arteries and veins, which were all very pale, but upon minute examination they were not thinner than natural, or in any way diseased. There was no effusion into the abdomen: the liver was very pale, but, as well as the kidneys and *renes succenturiati* of healthy texture, but only bloodless; the spleen large but natural; the stomach very much distended, and its villous coat in the cardiac half uniformly pale red; the gall-bladder contained much natural bile; the small intestines to the *ileum* very pale, but from this to the *cæcum* reddish, from containing reddish slime. From the cæcum the colour was progressively deeper through the *colon* into the *rectum*, in proportion as this part of the canal contained more mucus that was mixed with florid blood; the intestinal glands were more obvious in the small intestines, but accumulated and remarkably larger in the large intestines. In the free portion of the mesentery were many indurated glands, some of the size of a hazel nut, and of a firmer consistence than lard, but not so hard as those near the trachea.

In Horn's Archives for May and June 1820, there is a very elaborate paper on hereditary tendency to fatal hæmorrhagy by Dr Nasse of Bonn, in which he has collected the various examples of it that have been published, and added some original instances. It was first noticed by Albucasis in Alsaharavius Liber Theoreticæ necnon Practicae, translated into Latin from the Arabic M.S. by Paul Riccius, and printed at Augsburg 1519; and afterwards in Germany by the author of the Medical Ephemerides, Chemnitz 1793; the publishers of the selection of treatises on Practical Medicine. Vol. 22, Leipzig 1805; a review in the Gottingen Notices 1809; Consbruch in Hufeland's Journal 1810. Nasse himself, in his excellent Essay on Cyanosis in Reil's Archives 1811, and Meckel in the German Archives for Physiology, 1816. In America Dr Otto published concerning it in the Medical Repository, 1803, and quotes Dr Rush and Dr Boardley as having also observed it; an anonymous observer in the Philadelphia Medical Mnseum 1804, Dr Hay in the New England Journal, and the Doctors Buel in the Transactions of the Physico-Medical Society of New York, 1817.

The instance communicated to Dr Nasse by Dr Krimer is very striking. M. K. whose paternal uncle had been subject to epistaxis, had lost all her five brothers in early youth by hæmorrhagy, from trifling injuries, or without ascertained cause. She herself had five sons and two daughters. Of the sons four had died of hæmorrhage; but one, notwithstanding many dangerous attacks, was still alive when the history was written, and had two sons and a daughter, in whom the tendency had not appeared. A sister of M. K. had two sons and two daughters. One of the sons died in a few days after birth, of cramp, asphyxia, and blueness of the whole body. The other son was liable to bleedings and ecchymosis. None of the females of the family had any hæmorrhagic disposition.

***Contributions to the History of the Development of the Human Fœtus during the first three weeks after Conception.** From Dr Pockels Brunswick (Isis. Edited by Oken, and published at Jena Dec.* 1825.)—Accurate descriptions of the human ovum have generally been confined to a period at which important parts are no longer to be observed. From this cause have arisen the numerous disputes on the existence of the *vesicula umbilicalis*, on the presence of the intestines in this organ, the unsatisfactory views on the formation of the umbilical cord, as well as the complete misunderstanding of the most frequent cause of the death of the embryo, and the abortus which takes place in the earlier months of pregnancy. Within the last few years, from among more than fifty human ova in the first six weeks of pregnancy, Dr Pockels has had an opportunity of preserving four perfectly formed, which came away between the eighth and sixteenth day after conception. They were taken out of the *tunica decidua*, and the blood adhering between the layers of the chorion being removed, they were placed for some hours in fresh water. The examination was conducted under water, in the bottom of a black basin; the ova were so fastened, that they might not be moved by the water, and that minute observations with magnifying glasses might be made. The results of these observations are for the most part new, and, as Dr P. thinks, of no slight interest in the history of the development of the human embryo. They appear to him also so clear, as to supersede the necessity of diffuse demonstrations; and after enumerating what, up to the present time, is known on the subject, he briefly gives the results of his own investigations.

§ 1. The naturally formed ovum is, up to the 14th day after conception, of the size of a nutmeg or small walnut. It lies imbedded with the chorion in the *tunica decidua Hunteri*; and after opening this coat, it may be removed from its bed without tearing the layers of the chorion. There is no connexion whatever between the two coats through the medium of vessels. The cavity of the chorion contains a kind of red transparent fluid, of the consistence of the white of an egg; a delicate colourless membrane traverses the fluid in different directions, very similar to the structure of the *humor vitreus* in the eye. Of the coat which is said to line the chorion, in particular the *allantois* (*allantoides*), Dr P. never saw any clear trace.

§ 2. In this albuminous kind of fluid contained within the chorion, lies the small bladder-like amnion; in the first fourteen days after conception it is between the size of a pea and that of a bean, generally pyriform, sometimes however globular. From want of practice in these examinations, the amnion is, on account of its small circumference and its transparency, very easily removed from the surrounding fluid with the chorion, and then it is supposed that there is an ovum without an embryo; this was the case formerly with Dr P. In order properly to lay it bare, the albuminous fluid, with its membrane, must be removed with great care, by means of forceps and scissors, then a small white body, the embryo and the organs connected with it, together with the bladder of the amnion, become apparent. The amnion is usually, by means of its pyriform peduncle, slightly connected to the chorion at one point, through a little condensed membrane, and projects with its conical extremity into the albuminous fluid. The parietes are quite transparent, and it contains a clear fluid.

§ 3. The embryo appears to the naked eye, even in *ova* only fourteen days old, as a yellowish white body, scarcely a line (one-twelfth of an inch) in size; in the middle flat and pressed together; at both ends thicker and a little rounded, and has the consistence of jelly. The embryo lies, up to the 12th day after conception, external to the amnion, with its back in a flat depression, on the outer surface of the amnion, and connected to it by a clear cellular membrane; and at first it is so placed, as to admit of being raised from the amnion, without the amnion being opened. Its anterior surface is turned towards the chorion.

§ 4. Somewhere about the eighth day the embryo adheres by its back to the external surface of the amnion, and sinks in the progress of development into the cavity of the amnion. Towards the sixteenth day the embryo lies in the cavity of the amnion, close on the inner surface of this bladder, without a navel-string. The head and posteriors may be recognised as small white projecting knots, the back is still concave. During the advancement of the process of evolution, the embryo sinks farther into the cavity of the amnion; the septum on the anterior surface becomes elongated.

§ 5. Before, and also a short time after the passage into the amnion, there are two important organs in connexion with the embryo, which are also situated external to the amnion, viz. the *vesicula erythroides*, and the *vesicula umbilicalis*.

§ 6. The *veiscula erythroides*, a part of the human ovum, up to the present time quite overlooked, is a kind of small bladder pressed together, rather long, and of a pyriform shape, the rounded end of which rests on the amnion over the lower part of the embryo; and its smaller end opens into the abdominal part of the embryo, but a little before this it is slightly curved and dilated. In ova about eight or twelve days old, it is three times as long as the embryo; and in the fourth week after conception, is no more visible. It can very easily be raised with the embryo from the external surface of the amnion; sometimes, however, it is so firmly connected with the amnion, that it is difficult to separate the one from the other.

§ 7. The *vesicula erythroides* is transparent, of a milk-white colour, and in its parietes, which, in proportion to the size of the vesicle, are large, there can be seen a number of small red globules spread about in all directions, but which, however, soon lose their colour in spirits of wine. There are several groups in the form of a double string, interrupted however, in which at first the yellowish white globules can only be observed. Immediately after the entrance of the embryo into the cavity of the amnion, this string or cord (*Strang*) appears to the naked eye as a vermiform cord lying in the hollow of the *vesicula erythroides*, which, in the middle of the vesicula, divides itself into two canals, which are only to be observed with the help of very strong magnifying glasses, and thus passes into the abdomen of the embryo.

§ 8. On the passage of the embryo into the cavity of the amnion, the broad end of the *vesicula erythroides* separates itself from the amnion, follows the abdomen of the embryo that retracts into the sheath formed by the amnion, fills it quite out, and in the human ovum this *tunica erythroides* becomes the umbilical cord. This occurs about the third week; and in natural ova at this period after conception, the *vesicula erythroides* is no more to be found entire, lying on the amnion. On the contrary, one often sees in the natural ova, even at the fourth and fifth week, near the insertion of the umbilical cord, the broad but more obliterated end of the erythroides as a white lamella, opposite to the vesicula umbilicalis, resting on the amnion, and passing over into the sheath of the navel-string.

§ 9. The vermiform string (§ 7.) formed in the *vesicula erythroides*, draws itself up into several turns or folds towards the abdomen of the embryo; its simple rounded end remains in the VESICULA ERYTHROIDES; its two shrunk up canals surrounded by the *erythroides* go into the embryo; and the cavity of the *vesicula erithroides* becomes gradually obliterated from its broad end towards the abdomen of the embryo. On the abdomen there remains in the regular state, up to the tenth or eleventh week, a small hole of the VESICULA ERYTHROIDES, in communication with the cavity of the abdomen, and here are found very late some of the folds of the intestines, as is now sufficiently well known. About the 20th day these bent-up strings pass over into the embryo; and now the middle part of the embryo does not appear any more flat, the back no longer concave, the abdomen projecting and filled.

§ 10. The most accurate possible observation of the above mentioned gradations in the formation of this vermiform string, of its passage into the abdomen of the emybryo—its partial remaining behind in the abdominal extremity of the navel-string, as real intestines, places it, according to my opinion, beyond all doubt, that the fact discovered by OKEN, in the mammalia, also occurs in the human embryo; *viz.* that the intestines, in part at least, arise in the *vesicula orythroides*; in the human subject, not however out of the coats of the erythroides, but in its cavity.

§ 11. The *vesicula umbilicalis* is the second essential organ external to the cavity of the amnion, which stands in connexion with the embryo. It is a globular vesicle, rather larger than the embryo, rests a little above the top of the head of the embryo, loosely attached to the amnion. It is generally of a yellowish white colour, transparent, sometimes completely so, and clear as water, filled with a clear fluid, which does not become turbid in spirits of wine. I have never been able to observe a trace of red globules, and much less vessels in or upon it. It increases in size up to the existence of the navel-string, in proportion with the embryo; but then maintaining at the utmost, two lines in diameter, it does not increase any more.

§ 12. From the vesicula umbilicalis there proceeds an extremely fine canal, about one to three lines in length, appearing to the naked eye as a delicate white thread, close over the embryo into the vesicula erythroides, where this becomes curved and dilated. Immediately after the real existence of the intestines (§ 9), there may be seen, by the aid of powerful magnifying glasses, two delicate threads going from the canal into the vesicula erythroides, one of which extended towards the embryo, the other, however, appeared to extend in a contrary direction, but which, on account of the great fineness of the parts, I could not with certainty observe.

§ 13. After the existence of the umbilical cord, the *vesicula umbilicalis* unites with the external surface of the amnion; and as the amnion becomes more distended by fluid, the *vesicula umbilicalis* separates itself more and more from its original point of attachment from the insertion of the navel-string, with proportional elongation of the canal, which stands in connexion with the *vesicula erythroides*. The *vesicula umbilicalis* becomes obliterated and converted to a white round spot, and even exists in this state sometimes to the end of the third month of pregnancy. The fine canal, both of whose branches pass over with the folds of the intestine, through the *vesicula erythroides*, into the embryo, forms the omphalomeseraic vessels, which are to be seen as late as the ninth week; it becomes obliterated with the *vesicula umbilicalis*, and changed to a very fine white thread, that runs in the cord to the embryo, and divides itself at the mysentery into two branches. I have never been able, either in this canal or its branches, to discover a trace of red blood.

§ 14. Both organs, the *vesicula erythroides*, and *vesicula umbilicalis*, are essentially necessary to the development of the embryo. It is probable that the *vesicula umbilicalis* in the human ovum, is similar to one of those vesicular distentions, from which the tunica erythroides of animals springs, * but that in the human subject it it appears as a particular organ. It is probable that its fluid is employed in the development of the embryo, up to the formation of the umbilical vessels; that this process partly takes place by the vasa omphalomeseraica, which at the same time open with the formation of the instestine, into the *vesicula erythroides*. It is further likely that the proper umbilical vessels develope themselves into the parietes of the vesicula erythroides, arising either from the embryo, or taking their origin in the parietes of this organ.

§ 15. We find proofs of these views in the examination of unnatural human ova. When both organs are wanting, and perhaps from the commencement, then the embryo is found like a small stria, which is scarcely vissible, or as an irregular lamella (Blättchen), hanging by an extremely delicate thread, into the cavity of the amnion, which is unproportionally distended with fluid. The embryo is often so small, that it is sought for sometimes in vain in the large bladder; sometimes there is only a very small thread present, as the remains of the umbilical cord; sometimes also the diseased ovum is quite empty. If the *vesicula erythroides* adheres too firmy to the amnion, then the umbilical cord is like a short thin thread, or a sheath filled with saline matter, in which no trace of the folds of the intestines is to be found, but some vessel-like threads going from the embryo to the centre, the rudiments of the umbilical vessels are to be seen. The embryo has in these cases always an empty, either transparent, or pressed together, belly; it is always deformed, and appears only by its own peculiar powers, sparingly to vegetate, until it is expelled from the uterus. The *vesicula erythroides* is found in such diseased ova, proportionally very large, extending as a snow-white line, form the point of insertion of the navel-string, from one to two inches over the amnion, with the outer surface of which it is firmly connected. By the powerful distention of the amnion bladder, the *vesicula erythroides* in these cases, is as long as the thread of the *vesicula umbilicalis*. The *vesicula umbilicalis* disappears, according to Dr P.'s observations, either entirely, or it becomes obliterated with its canal too early, if the *vesicula orythroides* does not enter into the sheath of the umbilical cord. The only remnant which is then found of it, is a white lamella, adhering fast to the amnion, easily, however, overlooked, from which a thread extends itself towards the insertion-point of the navel string, but frequently before the point becomes lost in the amnion.

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* Oken Beyträge, Heft. I. Tab III. fig. 3. x. y.

§ 16. In the above cases the embryo developes itself either in a form not at all to be recognised as the embryo, or in a most imperfect degree; it remains, in respect to its size and form, in development, at a stand when the *vesiculæ umbilicalis et erythroides* cease to contribute to its support, and then perishes. The liquor amnii is not absorbed by it, the secretion from the coats of the ovum passes off, the ovum itself is expelled. Such diseased ova may remain months in the uterus, the woman believes, correctly, that she is in the 2d, 3d, or 4th month of pregnancy, and miscarries an ovum from two to four inches in diameter, which, according to the size and form of it, can be taken only for one from two to four weeks old. This premature expulsion of the embryo, arising from a defective development of the *vesicula erythroides* and *umbilicalis*, and unnatural connexion of the ovum, is evidently the most frequent cause of the numerous abortions in the early months of pregnancy. Slight mental agitations, corporeal exertion, which a pregnant woman bears without any bad consequence where the embryo is alive, become, in a defective formation of the ovum a not unfrequent predisposing cause of abortion.

§ 17. The conditions under which the situation and form of the embryo permit the *vesiculæ umbilicus* and *erythroides* to be found as mentioned above, will be very easily seen from the description already given. Dr P. remarks, that among thirty ova with embryos, which, from their size, might be looked upon as from 2 to 3 weeks old, only one in four was naturally formed in all its parts, and adapted for the above investigations. On account of the excessive number of irregularly formed ova, Osiander would stand a chance of winning the bet which he once offered to make, viz. that, in a given unopened ovum the *vesicula umbilicalis* would not be found. The examination of malformed ova, which, from the misproportion of their size to the smallness of the embryo, and the adhesion of the amnion with the chorion, may, prior to the opening, be recognised as such—the examination of these, Dr P. thinks, acquires fresh interest through the discovery of the faults in the formation, and the deductions which may be drawn from them respecting the functions of the different organs. Here is a new and a wide field for cultivation; and it appears to Dr P. more probable that we shall arrive at definite results in the history of the development of the fœtus rather by proceeding from the pathological to the healthy state, than by direct examinations of such tender gelatinous parts scarcely a line in size.

Note.

The *vesicula erythroides*, the existence of which in the early state of the human embryo, it is the object of Dr Pockel's paper to show, was noticed by Everard as existing in animals, in his work entitled Novus et genuinus hominis brutique animalis exortus;[*] and we find an engraving of it in Needham's Observationes Anatomicæ[†] under the name of corpus glandulosum Everhardi quod fatum utero connectit. Dr Pockels, however, is the first to assert its being present in the human embryo, a point which naturally requires further investigation before a decided opinion can be given on it. With respect to the *vesicula umbilicalis*, an engraving of which was first, we believe, given by Albinus[‡] in 1754, and afterwards by Wrisberg[§] in 1764, we will just state that its presence in the human embryo is now admitted by nearly all the Continental anatomists. The late Osiander in Gottingen certainly denied its presence; and, in the beautiful collection of human embryos which he left behind him, it is not to be seen; but it could only have happened that these were malformed, or that from some accident the *vesicula* was removed, for it has fallen to the lot of almost every anatomist in modern times to see it, and its existence is placed beyond all doubt.

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[*] Melib, 1661.
[†] Leyden, 1743.
[‡] Academ. Annot. Liber. 15. Leidæ.
[§] Descriptio Anat. Embryonis Obs. illus. Gott.

Accidental employment of Iodine as a remedy for Goitre. (Journal de Physiologie, Juillet 1825.)—The Number for last July of Magendie's Journal contains a few interesting observations by a French physician, M. Roulin, on the localities of goitre in South America. From his observations it appears, that none of the circumstances which have been successively supposed to be connected with the existence of goitre are any thing else than accidental. His inquiries have not hitherto led him to discover any invariable connexion between its prevalence, and the physical situation or manners of the country where it prevails. From a great number of facts, however, he has been led to conclude, that wherever it prevails, there is generally a much greater difference between the temperature of the air and of the water than where it is unknown. He was long at a great loss to account for the following fact. In the province of Maraquita the goitre was at one time unknown, and the women were even remarkable for their beauty. But it had become exceedingly common within fifty years, and in particular it was remarked, that many strangers took the disease after five or six years residence. Many unsatisfactory explanations occurred to him; but at length he discovered, that a liquor, produced by draining in linen bags the salt of certain mines, had been long used for curing the disease; but that for some years back a different salt from another quarter had been used instead of it. M. Roulin tried the efficacy of the first preparation; and his trials left no doubt that it cured goitre. On a subsequent occasion he got the different salts in question analyzed by Boussingault, who found that the useful liquor contained a considerable quantity of iodine, that there was a small portion likewise in the salt from which the liquor was drained, but that the salt of Zipaguira, which had been substituted for the former, did not contain any iodine.

Cure of a Deaf and Dumb Child. (Journal de Physiologie, Juillet 1825.)—A boy ten years old, who had been completely deaf from his infancy, was cured by M. Deleau, a Parisian physician, by a method which has been tried with success before, the forcible injection of air into the cavity of the tympanum through the Eustachian tube. The particulars of the operation are not given: but it appears to have been accomplished without causing any inconvenience, and has proved completely successful. The development of the voice has been very gradual and difficult, and attended with many very singular phenomena. Before the operation he could not hear any noise, however violent; his countenance was dull, his gait sluggish, and his manners stupid. On the restoration of his hearing he testified great delight, took great pleasure in listening to all kinds of sounds, and was thrown into extasy by a musical snuff-box. It was long before he had an accurate conception of the direction of sounds. He very soon began to imitate simple sounds with his voice, such as the vowels a, o, and u, and words containing them, such as *papa, tabac, du feu;* but the more complicated sounds cost him great effort, and he succeeded in pronouncing a few of them, not without extraordinary contortion of all the organs of speech. Very little progress was made in teaching him pronunciation by the sounds merely; but with the aid of the written signs he advanced much more rapidly. He has now been a year under tuition. "He can distinguish the characters of various sounds, knows when they come from a distance, avoids carriages and horses, opens the door when any one knocks, can appreciate musical rhythm, knows all the sounds of his language, can repeat by memory a certain number of easy phrases, and even reply to them, and finally executes by speech whatever his preceptor orders him." It is a curious circumstance, however, that he still continues to use signs only in communicating with other people on ordinary occasions; nay, instead of being gradually replaced by speech, his language of signs has become much more perfect and expressive. He entertains a profound contempt for the deaf and dumb children with whom he used formerly to associate.

Singular Case of disease of the Fifth Pair of Nerves. (Journal Physiologie, Juillet, 1825.—The indefatigable M. Serres, has just published a most interesting case of disease and partial destruction of the fifth cerebral nerve, confirming by an observation in man, the extraordinary phenomena witnessed by Bell, Mayo, and Magendie in their experiments on animals. A man 26 years old was admitted into the hospital of La Pitié for Epilepsy. He had at the same time a chronic inflammation of the right eye. The particulars of his case were not minutely attended to for some time, as every attempt made to question him tended to renew the epileptic fits. The inflammation of the eye became acute in December 1823; and though it was checked by proper treatment, the cornea became opaque, and the sight was lost. Not long afterwards it was also remarked, that the right eye was insensible. In June 1824 M. Serres, having been shown Magendie's experiments on the effects of dividing the fifth nerve in animals, was forcibly struck with the analogy subsisting between these effects, and the inflammation, opacity and insensibility of the eye in his patient; and in consequence was led to examine his state more minutely. The right eye was insensible to a blunt probe drawn over it; the right nostril was insensible to snuff, ammonia and ether; pepper placed on the right side of the tongue caused no sensation. On the contrary the left eye was quite sensible; with the left nostril the man could smell ether, ammonia, and snuff; and pepper put on the left side of the tongue caused salivation and a sense of heat and pungency. The movements of the jaw were entire; and the *sensation over the whole face and neck was perfect.* (This last fact is contradictory to every experiment hitherto made on the fifth nerve). A few days afterwards, the gums of the right side became red and swelled, and gradually receded from the teeth, which in consequence got loose in their sockets; and some days before the man died, he lost the hearing of the right ear. On dissection the right fifth nerve was found very much dieased. The ganglion Gasserii was enlarged, of a dull yellow colour, and the fibres were separated by serum; the nerves which proceed from it were in the same condition. The fibrils which form the muscular division passing under the ganglion were quite healthy; and the same was observed in the third branch or inferior maxillary, which, proceeding in part from the ganglion, is afterwards joined by the muscular fibrils. The muscular division of it was quite healthy, while the ganglionic portion had a yellow colour; so that the two portions were distinguished much better than they could be by dissection in the healthy subject. The trunk of the nerve before it forms the ganglion was so soft that it broke, although great caution was observed in raising the brain. The disease could be traced into the substance of the pons varolii from which the nerve arises; but the origin of the muscular division was healthy.

With the exception of the sensibility of the right side of the face there is not a single fact in this case which does not accord completely with those noticed by Magendie on animals. Not long after, two similar cases occurred in M. Serres's hospital, which he was enabled to cure in consequence of the light thrown upon them by this man's case and Magendie's experiments. So much for the affected sensibility and ignorant interference of those who have decried that physiologist's labours, and attempted to suppress physiological experiments generally.

Application of forcible injections for dilating stricture of the urethra, (Archives générales de Medecine, Octobre 1825)—M. Amussat proposes to reduce obstinate strictures, by injecting water with considerable force into the urethra. For this end he introduces a catheter, without a point, as far as the stricture, and having fastened to its upper end a caoutchouc bottle filled with warm water, proceeds to propel it by means of a species of tourniquet, which compresses the bottle. Scarcely any inconvenience is caused in cases of distended bladder, by the additional accumulation of fluid in it. When his paper was read before the Royal Society of Medicine at Paris, he exhibited an illustration of his method, by tying a thread round the urethra of a preparation, and then injecting water. The ligature was gradually untied, and the urethra dilated to its natural diameter.

Treatment of Tænia by Croton Oil.—(Annali Uuiversali di Medicina, Aprile e Maggio, 1825.)—An Italian physician, of the name of Puccinotti has lately recommended the employment of the croton oil as a successful remedy in bad cases of Tænia. He has not used it very often, but the results he has obtained justify the belief, that it is a useful remedy when even the oil of turpentine fails, as it is certainly also a preferable remedy in respect to the facility of its administration. The following is an abstract of the only one of his cases which he has given in detail:—A man, 28 years old, had been liable to tænia for six years; during which time a great variety of the most active remedies had been tried by a physician of skill. Puccinotti recommended him to use the croton oil in the dose of one drop, taken in a little beef-tea. The patient, following his instructions, lived sparingly for a couple of days, and took the medicine, while fasting, on the morning of the third day. It caused a slight sense of heat in the fauces, and, about an hour afterwards, began to operate as a mild laxative. He had eight stools in the course of the day, without pain, straining, or any uneasiness whatever, and discharged along with them a great number of fragments of tape-worm. The dose was repeated after a day's interval, and this time caused six stools, containing more fragments of the worm. For some days after that, the patient felt himself quite free from his former ailments, and convalesced rapidly. In a month, however, he was again taken ill with giddiness, tormina, and occasional discharge of joints of the worm. The croton oil was therefore resumed, with the same success as before; and it was repeated six times, with intervals of one or two days. He has not had any return of his complaints since.

Treatment of Poisoned Wounds by the Application of Cupping-glasses.—(*Archives Générales de Médecine, Septembre & Octobre* 1825.)—At the meeting of the Royal Academy of Medicine of Paris, on the 9th of August last, Dr Barry, an English physician, resident in the French metropolis, read an account of some experiments on the effect of applying cupping-glasses to poisoned wounds. According to Dr Barry, if a mortal dose of strychnia, or hydrocyanic acid, the most deadly poisons with which we are acquainted, be introduced into a wound after the blood has ceased to flow, the usual effects will be prevented as long as the suction is kept up; nay, if the cupping-glasses be applied after the symptoms of poisoning have commenced, the animal will recover. A committee of the Academy was appointed to witness and report on the experiments of Dr Barry; and their report confirms all his statements. The following is an abstract of the most remarkable experiments: Eight grains of arsenic were introduced into a wound in the thigh of a dog. In four hours he was very ill; and he died in fifteen hours. On another dog, under the same circumstances, a cupping-glass was applied 45 minutes after the arsenic, and in four hours there was not the slightest appearance of ailment. Six drops of diluted hydrocyanic acid, containing 1½ of the pure acid, were dropped into a wound in a rabbit, and a glass applied immediately afterwards. In twelve minutes, no symptom of poisoning having appeared, the glass was taken off. The animal was immediately seized with violent convulsions, and soon appeared dead. It revived on the reapplication of the cupping-glass. After twelve minutes more had elapsed, the glass was again taken off, and the same symptoms recommenced. On then applying it twelve minutes longer, it was withdrawn permanently, and the animal recovered. A rabbit poisoned under the same circumstances, without the application of the cupping-glass, died in two minutes. One grain of the upas-tieuté was thrust deep into a wound in a rabbit, and a cupping-glass applied immediately. The results were analogous to those obtained with the hydrocyanic acid. On two occasions, half an hour and three-quarters of an hour after the introduction of the poison, the removal of the cupping-glass was followed by the appearance of the peculiar symptoms of poisoning with strychnia; and each time the symptoms receded when the glass was replaced. Dr Barry thinks these experiments prove, that not only the absorption of the poison is prevented, but likewise what is absorbed previously to the application of the cupping-glass is withdrawn from the circulation; and some have seen in them a proof that certain poisons, previously supposed to act through the blood, really act through means of the nerves. In our opinion, neither supposition is well founded. It is absurd to talk of poison being drawn to the wound after being distributed through the whole circulating mass of the blood; the symptoms disappear, because what is absorbed is either excreted or decomposed, and because, farther absorption being prevented, the poison cannot accumulate in sufficient quantity to prove fatal. By intermissions in the application of the glass, the whole poison is absorbed in separate small portions, and discharged from the system in like manner; so that the symptoms do not become dangerous in degree. Barry's experiments, so far from upholding the doctrine of poisons acting by nervous sympathy, appear, in our opinion, to disprove it. We can easily understand how the suction caused by cupping-glasses will prevent absorption. We cannot, on the other hand, perceive how it could prevent the poison from lying in contact with the nerves, which is all that can be requisite to produce the necessary impression. These experiments are deeply interesting, and, in a practical view, highly important.

Dr Morris's Case of Stone in the Bladder, with a new method of relieving the sufferings occasioned by that complaint.—The following particulars we have been permitted to extract from a highly interesting communication by Dr Morris of Canada. The Doctor's observations derive additional interest from the circumstance of his having been himself the subject of the disease he describes.

Many months since, our author states, he was attacked with symptoms of calculus. He had "violent and excruciating pain in his right ureter, increasing at intervals for about 12 hours, with a profuse discharge of blood." After this the calculus "dropped into the bladder."

To set the nature of the affection out of all doubt, the Doctor had a sound introduced by an eminent surgeon. The result of this operation was complete confirmation of the conclusion which the previous symptoms had led the patient to adopt—and now an opportunity presented itself of putting to the test of experience, on his own person, a speculation which the Doctor had entertained for a considerable time anteriorly to the supervention of the symptoms above mentioned. Different considerations had inclined Dr M. to the opinion, that the introduction of a lubricating fluid into the bladder would be productive of ease and advantage. This experiment, therefore, he now determined on trying on his own person, and he at length put the project in execution. "I first took care," says Dr Morris, "to rid myself of the contents of my bladder; this I had no sooner accomplished than, with a large syringe, I injected (through a small leaden tube reaching to the sphincter, no further) about two ounces of the oil (cold-drawn castor-oil), and I cannot express to you my feelings occasioned by the change which took place upon the moment of its introduction, for *it seemed as if a new lower half had been given me.*"—"The absence of former symptoms still continuing, I went to bed, and can safely say that I had not known, for some time previous, the pleasure of a sound and uninterrupted sleep. Latterly I never awoke without a wish to make water, and the morning following was the first exception to it. When I did obey the call, I took care, finding that the oil came last, to leave as much within the bladder as I could. This I had little difficulty in effecting, as it does not dispose the bladder to contract as other fluids do."

After this the bladder was constantly supplied with two or three ounces of oil, and under this treatment every symptom of irritation vanished, and during two months, namely, from the first introduction of the oil up to the date of the Doctor's departure for Canada, no one symptom reappeared to remind him of the existence of the calculous concretion.

Previously to using the injection, Doctor Morris had "*tried the introduction of Weiss*'s instruments, and endeavoured to enlarge the passage with the hope of expelling it, knowing that the stone was not large; but "whether it was from any defect in the form of the instrument I employed, says Dr M., or from my own want of skill in the management of it, which latter is the more likely, I cannot say. I can only affirm, that with all the delicacy I could master I could not obviate the disagreeable feeling its presence occasioned. The contraction of the bladder in its endeavours to expel it were intolerable, and with the extension of the parts and irritation it left, it gave rise to an incontinence of urine of three months duration."

An interesting remark of Dr Morris's to which we cannot neglect calling the reader's attention, is, that after the injection of the oil, and during the experiment, he has observed particles of sand evacuated. Before the use of the injections the Dr does not appear to have detected any gravel passing with the urine. He is disposed, therefore, to believe, that the appearance of sand is to be attributed to a softening of the stone by the oil; and, taking into account the absence of symptoms of irritation in the kidney, ureter, and bladder, that view of the matter is perhaps more probable than any other. Of the further progress of Dr Morris's experiment, we shall be very glad to receive any information the Doctor may be enabled to communicate.

Should the result of any repetition of Dr M.'s experiment, that may be made on this side of the Atlantic, reach us, we shall of course make it known, on the earliest convenient occasion, to all readers of this Journal.

Letter from Dr Ballingall to Dr Duncan junior, communicating a Case of Hernia Infantilis.

I have much pleasure in transmitting you the following brief account of the case of *Hernia Infantilis*, on which I operated about ten days ago. Although this species of hernia may not be unfrequent, it has very rarely become the subject of operation; and the following case may therefore be thought worthy of notice in your Journal.

WILLIAM BOWMAN, æt. 13, was brought into the hospital on the afternoon of Sunday the 5th instant, with a hard inflamed tumour in the right groin, bearing the characters of strangulated hernia. The history given of it by his relations, who accompanied him, was imperfect and somewhat contradictory; but it was distinctly stated to have been strangulated since the preceding Tuesday, frequent attempts having been made to reduce it in the interval without success.

Although the tumour was a good deal inflamed and tender, with some degree of lividity of the scrotum, the other symptoms were not urgent, and I therefore requested the house surgeon to have a purgative glyster administered, to let the boy be placed in a warm bath, and to make gentle efforts for the reduction of the tumour by the taxis. These not proving successful, I proceeded to the operation at four o'clock, under the impression that I had a case of common congenital hernia to deal with.

" The external incision, and first steps of the operation, were performed in the usual manner. After dividing several layers of fascia, the tunica vaginalis testis was exposed; and on laying this freely open, the testicle came into view, having the hernial tumour situated above it, still covered by its proper sac. On dividing this with great care, a very singular appearance presented itself. It was at first thought to be a portion of the omentum lying over the intestine, or a part of the colon with its appendices epiploicæ; but on more accurate examination, it proved to be a newly formed substance, composed of coagulable lymph. Some portions of this newly formed substance adhered firmly to the gut, others were removed with the fingers; and on the intestine being exposed, it appeared of a dark chocolate colour; At first it felt tense, but immediately became flaccid; and the stricture in the external ring being removed, it was easily reduced.

The functions of this boy's bowels were restored in a few hours after the operation. He eat some pottage on the following morning for breakfast, and has since continued without any unfavourable symptom whatever.

Queen Street, 15*th March*, 1826.

Medical Officers in His Majesty's Naval Service.

Victualling Office, 23d *February* 1826.

The Right Honourable the Lords Commissioners of the Admiralty having been pleased to direct, "that no person be admitted to be a Candidate for the situation of Assistant Surgeon in the Royal Navy, who shall not produce a Certificate from one of the Royal Colleges of Surgeons of London, Edinburgh, and Dublin, of his fitness for that office; nor, for that of Surgeon, unless he shall produce a Diploma, or Certificate, from one of the said Royal Colleges, founded on an examination to be passed subsequently to his appointment of Assistant Surgeon, as to the Candidate's fitness for the situation of Surgeon in the Navy; and that in every case the Candidate producing such Certificate, or Diploma, shall also undergo a further examination before the Medical Commissioners of the Victualling Board, touching his qualifications in all the necessary branches and points of Medicine and Surgery for each of the steps in the Naval Medical Service;" The Commissioners for Victualling His Majesty's Navy, &c., do hereby signify, for the information of those persons to whom it may relate, that these regulations and directions will be strictly adhered to in future; and further, that previously to the admission of Assistant Surgeons into the Navy, it will be required that they should have received a classical education, and possess in particular a competent knowledge of Latin; also,

That they should have served an apprenticeship, or have been employed in an Apothecary's shop for not less than two years.

That their age should not be less than 20 years, nor more than 26 years.

That they should have attended an Hospital in London, Edinburgh, Dublin, or Glasgow, for 12 months; and

That they should have attended Lectures, &c. on the following subjects, for periods not less than hereunder stated, viz.

Anatomy	18 Months.
Surgery	18 —
Theory of Medicine . .	12 —
Practice of ditto . . .	12 —
Chemistry . . .	6 —
Materia Medica . . .	6 —
Midwifery . . .	6 —
Actual dissections of the human body	6 —

Although the above are the only qualifications which are absolutely required in Candidates for the appointment of Assistant Surgeon, a preference will be given to those who, by possessing a knowledge of diseases of the Eye, and of any branch of science connected with the profession, such as Botany, Medical Jurisprudence, Natural Philosophy, &c. appear to be more peculiarly eligible for admission into the Service.

It is also to be observed that, by the rules of the Service, no Assistant Surgeon can be promoted to the rank of Surgeon until he shall have served full three years in the former capacity; and the Board have resolved that not any Diploma or Certificate of examination from either of the aforesaid Royal Colleges, shall be admitted towards the qualification for Surgeon, unless the Diploma or Certificate shall be obtained on an examination passed after a period of not less than three years service as Assistant Surgeon.

By command of the Board,

M. WALLER CLIFTON.

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